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ON UNDERSTANDING LOGICALLY COMPLEX SENTENCES

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An experimental investigation was made into the meaning of eight types of *doubly-quantified* sentence, e.g. "*Every* medicine cures *some* disease," "*Some* disease is cured by *every* medicine." All the sentences were ambiguous, depending upon the interpretation of the quantifiers. Subjects classified diagrams representing different specific situations as truthfully or falsely described by the sentences. The classifications revealed that the order of occurrence of the two quantifiers had a crucial effect, causing active and correlative passive to receive different interpretations. This suggested that in the process of understanding an ambiguous sentence a bias towards one intepretation may be created by word order.

Introduction

There is a controversy about whether the active and passive voice are *always* synonymous (cf. Chomsky, 1957, 1965; Katz and Postal, 1964; Ziff, 1966; Katz and Martin, 1967). It seems to have arisen, in part, because of a failure to distinguish probable interpretations from possible interpretations. Consider, for example, this pair of *quantified* sentences:

(1) *All* philosophers have read *some* books.

(2) *Some* books have been read by *all* philosophers.

Both sentences, isolated from any context, are ambiguous in the same way. They mean either (a) all philosophers have read some books *or other*, or (b) all philosophers have read some books *in particular*. But, although both sentences have the same possible interpretations, it seems intuitively that these interpretations are not equally probable—they are not equally privileged. Sentence (1) seems more likely to receive interpretation (a), and sentence (2) seems more likely to receive interpretation (b). One aim of the present experiment was to test whether there was this difference in the privileged interpretations of a variety of logically complex sentences.

Is there any explanation why these sentences should have different privileged interpretations? A preliminary answer is provided by considering their symbolic form within the quantificational calculus (cf. Ch. 4 of Suppes, 1957). The existential quantifier (Ex) stands for "there exists at least one x . . .," and the universal quantifier (x) stands for "for any x. . . ." Let x range over philosophers,

y range over books, and R stand for the relation of "reading," then it is possible to express the two interpretations of the sentences in the following form:

(a) $(x)(Ey)(xRy)$ Every philosopher has read some books *or other*.

(b) $(Ey)(x)(xRy)$ There are some books *in particular* that every philosopher has read.

These symbolic interpretations have their quantifiers in the same order as the actual sentences for which they are assumed to be privileged. Hence, an explanation for the effect is that word order determines which interpretation is privileged. What comes first in a sentence receives more emphasis than what comes later (cf. Johnson-Laird, 1968*a*, *b*) and the first quantifier will, in the technical sense, *bind* the second quantifier. This is not a complete explanation, however, until an analogue for the technical notion of binding is found in everyday language. Such an analogue is revealed by an examination of the expressions which realize the existential quantifier, e.g. "some," "at least one," "several," "a few," etc. All of these expressions are essentially ambiguous. Compare the probable interpreta-

TABLE I

Twelve basic quantified "two-place" relations

1.	$(x)\ (Ey)\ (xKy)$	Every man knows some woman (or other).
2.	$(Ey)\ (x)\ (xKy)$	Some woman (in particular) is known by every man.
3.	$(y)\ (Ex)\ (xKy)$	Every woman is known by some man (or other).
4.	$(Ex)\ (y)\ (xKy)$	Some man (in particular) knows every woman.
5.	$(x)\ (Ey)-(xKy)$	Any man does not know some woman (or other).
6.	$(Ey)\ (x)-(xKy)$	Some woman (in particular) is not known by any man.
7.	$(y)\ (Ex)-(xKy)$	Any woman is not known by some man (or other).
8.	$(Ex)\ (y)-(xKy)$	Some man (in particular) does not know any woman.
9.	$(x)\ (y)\ (xKy)$	Every man knows every woman.
10.	$(x)-(Ey)\ (xKy)$	No woman is known by any man whatsoever.
11.	$(Ex)\ (Ey)\ (xKy)$	Some man knows some woman.
12.	$(Ey)-(x)\ (xKy)$	Some woman (in particular) is not known by every man.

(Ej) denotes the existential quantifier, (j) denotes the universal quantifier, and "—" denotes negation. It is assumed that x ranges over men, y ranges over women, and K denotes the relation of "knowing".

tions of "John has several girl friends" and "John always has several girl friends"; in the first sentence the reference is to some girls in particular, but in the second sentence it is to some girls or other. Where there is only a single quantifier in the sentence the ambiguity is trivial, but where there are two or more quantifiers it may be crucial. When "some" occurs first it will receive a greater emphasis according to the hypothesis about word order. This emphasis is more in accord with "some in particular" than "some or other," since emphasis is given to particulars rather than to vague generalities. The converse argument applies where the existential quantifier comes after the universal quantifier. This explains the privileged interpretations of sentences (1) and (2). But the explanation also applies to other sentences where intuition is a less certain guide, and by examining these sentences experimentally the theory is put to a more stringent test.

There are 12 basic quantified "two-place" relations, and these are listed in Table I in a symbolicm for and in a more or less idiomatic translation ("—" is the sign for negation). There are other logically equivalent formulations of these relations, any one of which may be expressed in English in a variety of ways. But the material selected for investigation was of the same form as sentences (1) to (8). This material was chosen in order to investigate three variables: *polarity*—whether the sentence is affirmative or negative, *voice*—whether the sentence is active or passive, and *word order*—whether "some" is in the grammatical (surface) subject or object of the sentence.

There is another reason for investigating these sentences. A traditional problem in the psychology of reasoning is to determine the influence of linguistic form upon judgements of validity (cf. Woodworth and Sells, 1935; Sells, 1936). The experiments typically utilize syllogisms with singly-quantified sentences; but inferences may also be made with doubly-quantified sentences and these, too, may be influenced by linguistic form. Experiments could utilize either the unambiguous but stilted jargon of the logician ("For any man, there exists a woman such that the man knows the woman") or the idiomatic but ambiguous sentences of everyday discourse ("Every man knows some woman"). What, in fact, is needed is a preliminary study of the idiomatic material, since its very ambiguity may be a significant factor in reasoning. The present experiment was intended as a preliminary to a further study of inference.

Method

Task

Each subject was presented with a series of sentences. For each one, ten diagrams—representing different states of affairs—had to be classified as truthfully or falsely described by the sentence. The diagrams were chosen so that their classification would elucidate the subjects' intepretations of the sentences.

Design

Each subject was his own control and received the eight different forms of sentence. In order to reduce residual effects from one item to the next, each of the sentences had a different lexical content. Such effects seemed likely to be greater for logical form than for lexical content, hence, as a procedural variable, a series of 24 counterbalanced orders of presentation was derived from three 8×8 Williams' squares (cf. Edwards, 1963, p. 275). A separate order for the lexical material was assigned to each Williams' square; this provides only a crude control for effects associated with the lexical material, but it does provide an adequate control for any interaction between logical form and lexical content. The order of presentation of the diagrams was randomly determined for every trial.

Materials

The eight logical forms were those of sentences (1) to (8) except that the words in parentheses were omitted. The universal quantifier differs between the affirmative and negative sentences. In a pilot study, the use of "any" gave rise to a peculiar difficulty especially with the affirmative sentences. Subjects tended to regard a sentence like "Any medicine cures some disease" as true if a single medicine, chosen at random by the subject, cured some disease. Thus, the truth of the sentence depended upon "the luck of the draw." "Any" was accordingly used only where there was no alternative, that is, in the negative sentences, since "Some medicine does not cure *any* disease" differs in meaning to "Some medicine does not cure *every* disease."

The lexical materials are stated below in the same logical form throughout:

(a) Every man knows some woman.

(b) Every car overtakes some bus.

(c) Every job requires some skill.

(d) Every child loves some toy.

(e) Every brother hates some sister.

(f) Every liquid dissolves some solid.

(g) Every medicine cures some disease.

(h) Every animal possesses some instinct.

These materials were selected so as to vary the types of noun (e.g. human, abstract, etc.) and the reversibility of the sentences, i.e. whether the two nouns could be interchanged without producing a semantic anomaly (cf. Slobin, 1966).

The diagrams consisted of 3 × 3 matrices with each cell containing either a "1" or a "0": a typical matrix is shown in Figure 1. The ten matrices are shown in Table II. To understand the way the matrices have to be interpreted, imagine that they are paired with the sentence "Some woman is known by every man." Each row in the matrices then represents a man, and each column represents a woman. A "1" indicates that the man in whose row it is *knows* the woman in whose column it is. A "0" indicates that the man does *not* know

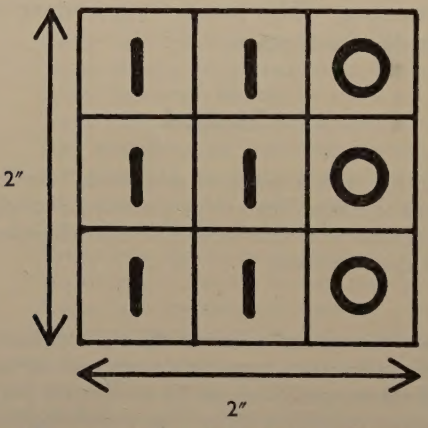

FIGURE 1. A typical matrix diagram.

the woman. Thus, the situation depicted in, say, matrix (8) is likely to be classified as truthfully described by the above sentence.

The choice of the particular ten matrices was based on two grounds. First, they were chosen as a representative sample from the complete set of such matrices, ignoring rotations or other transformations which had no effect upon how the matrices ought to be classified. Second, they were chosen so that the likely interpretations of sentences would be clearly reflected in the *pattern* of the subjects' choices. Three pairs of matrices, (3) and (8), (4) and (7), and (5) and (9), were chosen so that the logical classification would be the same for both members of the pair. These mutually redundant pairs provide a simple test of whether an interpretation is within the quantificational calculus.

The rows of the matrices always represented the logical (deep structure) subject and the columns the logical object of the sentences. This was likely to enhance any effect due to voice, but it was nevertheless left uncontrolled so as to minimize the difficulty of the task. To help subjects to identify rows and columns, a large "backing" card (9 in. × 6 in.) on

which there was an appropriately labelled blank matrix was on view throughout the classification. Eight backing cards were prepared: one for each set of lexical material, with the labels on the rows and columns typed in capitals.

Each of the 64 sentences (8 logical forms × 8 lexical contents) was typed on a plain 5 in × 3 in. card. The matrices, 2 in. × 2 in., were drawn in heavy ink at one end of a similar card, so that the subject could hold a card in his hand and clearly see the matrix.

Predictions

The nature of the predictions was slightly unusual in so far as there were two levels within them. Table II gives what will be termed the *privileged* classifications of the matrices. They rest on the assumption that subjects will give each sentence its privileged interpretation and

TABLE II

Ten matrix diagrams and the privileged classifications for the basic quantified relations

	1	2	3	4	5	6	7	8	9	10
	000 000 000	100 000 000	100 100 100	100 010 001	111 000 000	111 100 100	110 101 011	110 110 110	111 111 000	111 111 111
1. Every man knows some woman	0	0	1	1	0	1	1	1	0	1
2. Some woman is known by every man	0	0	1	0	0	1	0	1	0	1
3. Every woman is known by some man	0	0	0	1	1	1	1	0	1	1
4. Some man knows every woman	0	0	0	0	1	1	0	0	1	1
5. Any man does not know some woman	1	1	1	1	0	0	1	1	0	0
6. Some woman is not known by any man	1	1	1	0	0	0	0	1	0	0
7. Any woman is not known by some man	1	1	0	1	1	0	1	0	1	0
8. Some man does not know any woman	1	1	0	0	1	0	0	0	1	0
9. Every man knows every woman	0	0	0	0	0	0	0	0	0	1
10. No woman is known by any man	1	0	0	0	0	0	0	0	0	0
11. Some man knows some woman	0	1	1	1	1	1	1	1	1	1
12. Some woman is not known by every man	1	1	1	1	1	1	1	1	1	0

Note: Each row of a matrix represents a man, and each column represents a woman. A "1" under a matrix indicates that the matrix is classified as truthfully described, a "0" indicates that it is classified as falsely described. For convenience, the privileged classifications for sentences 9 to 12, not actually used in the experiment, have been stated.

then classify the matrices in a strictly logical fashion. These predicted patterns of classification may be summarized in the following way:

Sentences	*Matrices truthfully described*	*Matrices falsely described*
(1)	At least one "1" in every row.	At least one row of "0's."
(2)	At least one column of "1's."	At least one "0" in every column.
(3)	At least one "1" in every column.	At least one column of "0's,"
(4)	At least one row of "1's,"	At least one "0" in every row.
(5)	At least one "0" in every row.	At least one row of "1's".
(6)	At least one column of "0's".	At least one "1" in every column.
(7)	At least one "0" in every column.	At least one column of "1's.".
(8)	At least one row of "0's."	At least one "1" in every row.

It is clear that subjects can depart from the privileged classifications in a number of ways: they may produce the unprivileged classification (i.e. the one appropriate to the correlated sentence of the opposite voice), or a classification which is neither privileged nor unprivileged although still within the quantificational calculus (some Boolean function of the patterns in Table II), or finally a classification which is outside the quantificational calculus altogether.

Although subjects were expected to produce the privileged classifications, they were not expected to perform in every respect like trained logicians. Hence, a series of lower level predictions were made concerning departures from the privileged classifications: certain aspects of the sentences would cause difficulty, i.e. longer classification times and departures from the privileged classifications. It was predicted (i) that negatives would cause more difficulty than affirmatives. Negatives customarily cause difficulty (Wason, 1959, 1961) unless they occur in plausible contexts (Wason, 1965). It was also predicted (ii) that passives would cause more difficulty than actives, although this would be less than the effect caused by negatives (cf. McMahon, 1963; Gough, 1965; Slobin, 1966). Finally, it was predicted (iii) that the word order of the quantifiers would have an effect. The privileged interpretation should be easier to make when "some" is in the grammatical subject than when it is in the grammatical object. The argument underlying this prediction is that when "some" occurs in the object, it is taken as "some or other" almost by default, i.e. "some" is *not* in the subject therefore it does not mean "some in particular". Accordingly, this should lead to a greater difficulty in classifying the matrices.

Subjects

The subjects were 24 first-year undergraduates (14 male, 10 female) in the Department of Psychology, University College London. They were native-speakers of English but they were not familiar with the quantificational calculus.

Procedure

The subjects were allocated to a particular combination of logical and lexical material, and tested individually.

The subject sat at the end of a table which was screened off from the experimenter's part of the table. The experimenter read the instructions to the subject. These explained that the task was to determine what situations were truthfully (and falsely) described by a series of sentences. Each situation would be depicted by a diagram. A matrix was then introduced and the way it was to be interpreted was described in detail. The classification procedure was explained, and the subject was told that he could not ask any questions after the backing card for a sentence had been described. The subject was also told that he would be timed but that this must not disturb him: "I don't want you to race blindly through the task. Just imagine that somebody else has said the sentence to you, make up your mind what you think it means, and then make your classification. Make sure that your classification truly represents your considered opinion about the meaning of the sentence." Finally,

the subject was warned not to let his judgements be influenced by whether he thought the sentences were *really* true (or false).

The experimenter produced the appropriate backing card and the matrices, and described the lexical content of the sentence. The stopwatch was started as soon as the sentence was placed in front of the subject; it was stopped as soon as the last card was classified. The subjects were allowed to change their classifications, and the times recorded include intervals taken up by this. Between trials the experimenter recorded the time and the classifications, and re-ordered the matrices for the next trial, all of which took up an inter-trial interval of about 25 sec.

Results

Classifications

The proportion of subjects making the privileged, unprivileged, and other classifications for each form of sentence is shown in Figure 2. There was no reliable difference in performance with the different lexical materials and no

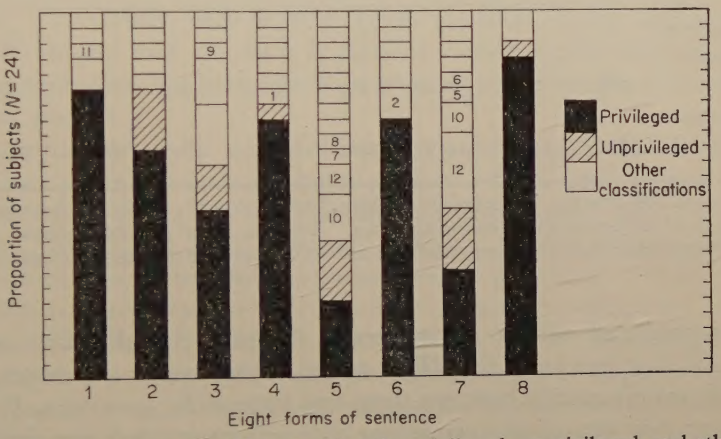

FIGURE 2. The proportions of subjects making the privileged, unprivileged, and other classifications for each form of sentence. (A number in a column indicates that the classification was privileged for the corresponding sentence e.g. 3 subjects gave sentence 5 the privileged interpretation of sentence 10.)

interaction between them and the logical forms, hence the analysis was on pooled data. The modal classification for each of the eight logical types was the predicted privileged one. The average number of privileged classifications per subject was 4·7 (out of a maximum of 8); since the probability of a given classification by chance is less than one in a thousand, it is evident that there was a very reliable bias in favour of the predicted classifications.

Since a major concern was whether the classifications were privileged or unprivileged, each classification was scored with respect to both of these interpretations. The higher score was taken as an index of which interpretation had been made. This made it possible to take all the data into account and to assign to each subject a number, ranging from $+8$ to -8, as a function of the degree to which he made the privileged interpretations. There was an overall mean of $+5{\cdot}2$, and all 24 subjects had a positive score, which is highly significant on a sign test ($P = 0{\cdot}5^{24}$).

Departures from the privileged classifications

The maximum number of departures from the privileged classification which could be made on any trial was ten, since there were ten matrices to be classified. The mean number of such departures for each of the eight forms of sentence is shown in Table III, along with the classification times. An analysis of variance ($2 \times 2 \times 2$ fixed factors with repeated measures) indicated that both polarity and

TABLE III

Mean number of departures from the privileged classifications and mean classification times for the eight forms of sentence

	Affirmative				Negative				
	Subject		Object		Subject		Object		
	Active	Passive	Active	Passive	Active	Passive	Active	Passive	
Sentence no.	(4)	(2)	(1)	(3)	(8)	(6)	(5)	(7)	Overall
Mean departures from privileged classifications	0·8	0·9	0·4	1·1	0·3	0·9	2·4	2·3	1·1
Mean classification times (sec.)	40·2	44·6	44·5	52·4	50·7	48·0	64·5	74·4	52·4

Note: the word order factor, indicated by "subject" and "object", refers to whether "some" was in the grammatical subject or object of the sentence. The maximum possible number of departures from the privileged classification on any trial was 10. The sentence numbers quoted are those used in Table I.

word order produced reliable differences in the predicted direction, with $F_{1,161}$ ratios of 11·2 ($P < 0{\cdot}01$) and 16·5 ($P < 0{\cdot}001$), respectively. However, there was also a significant interaction between these two factors ($F_{1,161} = 19{\cdot}1$, $P < 0{\cdot}001$). Neither voice nor any of the remaining interactions yielded significant effects. Departures from the privileged classifications showed no signs of diminishing with practice.

If the subjects had performed as predicted in Table II, they would have classified an average of 5 out of 10 matrices as true on each trial. In fact, the overall mean was 4·5 matrices classified as true: 18 subjects had averages less than 5, three subjects had averages of greater than 5, and the remaining three subjects had averages of exactly 5. Hence, there was a significant bias ($P < 0{\cdot}002$ on a sign test) in favour of classifying a matrix as falsely described by the sentence. Performance did not differ reliably between matrices: they all tended to be classified as true too few times.

Classification times

The mean classification times, from the moment the sentence was presented until the classification was complete, are shown for the eight forms of sentence in Table III. An analysis of variance indicated that there were significant effects due to polarity ($F_{1,161} = 9{\cdot}2$, $P < 0{\cdot}01$) and word order ($F_{1,161} = 8{\cdot}1$, $P < 0{\cdot}01$). Neither voice nor any interactions yielded reliable effects, though Table III indicates a

trend in the direction of an interaction between polarity and word order. There was a marked learning effect in the classification times of all eight forms of sentence.

Qualitative results

Relatively few classifications were outside the quantificational calculus, i.e. involved an inconsistent classification of a mutually redundant pair of matrices. Sixteen subjects were responsible for a total of 31 such classifications (16·1 per cent of all classifications). This percentage, however, constitutes just under half of those interpretations which were neither privileged nor unprivileged. It was often the case that a plausible account could be given for the interpretations outside the quantificational calculus. For example, three subjects presented with sentence (3) of the form "Every woman is known by some man" produced the privileged classification except that they classified matrix (9) as falsely described by the sentence. It seems they had taken the some *or other* interpretation so much to heart that they refused to countenance the regularity of two particular men knowing all the women. Conversely, with sentence (4) of the form "Some man knows every woman," one subject took the some *in particular* interpretation so strictly that his classification represents one particular man knowing every woman and no other man knowing any women.

The interpretations which were within the quantificational calculus but neither privileged nor unprivileged are harder to explain. Some of them seem to be no more than mistakes, e.g. (1) "Every man knows some woman" being interpreted as (11) "Some man knows some woman." Others, particularly those involving Boolean combinations of the basic sentences, seem to have some obscure motivation, e.g. four subjects interpreted (3) "Every woman is known by some man" to imply in addition that (1) "Every man knows some woman." This is, perhaps, a case of subjects' judgements being influenced by the real world. Figure 2 shows that sentences (5) and (7) tended to receive a variety of alternative interpretations; as we shall see, these were *not* primarily due to misinterpretations of the sentences.

Subjects rarely interpreted "some" to imply "not all." Thus, for example, "Some medicine cures every disease" was deemed quite consistent with the situation in which every medicine cures every disease. This *logical* performance is in contrast to the view of Chapman and Chapman (1959) that, with singly quantified sentences, "Some A is B" is usually taken to imply "Some A is not B." Yet in ordinary discourse "some" often does imply "not all"; thus it may be that this implication is less strong with doubly quantified sentences. A related finding was that "some N"—where N stands for a singular noun—was rarely interpreted to mean just a single entity. One such case has been mentioned, there were only two others.

Introspective reports

No subject spontaneously complained about the ambiguity of the sentences, though it was evident that at least ten subjects had noticed the ambiguity of "some." A number of subjects were able to state the features of the sentences which had caused them difficulty. These reports varied from general statements referring to "the negative sentences" or "the sentences with 'any' and 'some' " to the more

sophisticated reports of the five subjects who said that "some" could be either singular or plural. This distinction was made even when it was clear that the subject had never interpreted "some" as singular. It represents perhaps the nearest that subjects could get to stating the distinction between some *in particular* and some *or other*.

Only two subjects mentioned the passive voice; one subject said that he was surprised because some of the sentences could have been more simply expressed in the active voice, the other subject had "switched the sentences round when they were passives" though, in fact, this subject did *not* interpret the passives as synonymous with their active correlates. There were just seven occasions (out of 96) on which an active and correlative passive received the same interpretation.

Discussion

The task which subjects had to perform in the present experiment was difficult. It involved interpreting logically complex sentences with respect to a series of rather abstract situations. Nevertheless, the subjects consistently made the privileged classifications, with active and correlative passive sentences receiving different interpretations. The departures from the privileged classifications indicate that, as predicted, there was more of a consensus about the meaning of affirmative than negative sentences. There was, again as predicted, more of a consensus when "some" was in the grammatical subject than when it was in the grammatical object. Unfortunately for the assumptions on which these predictions were based, there was an unexpected interaction between the two factors: word order had an effect on the departures from the privileged classifications only in the case of negative sentences (cf. Table III).

The main question which the interaction raises concerns sentences (5) and (7), e.g.:

(5) Any child does not love some toy.

(7) Any toy is not loved by some child.

Why did these sentences receive more diverse interpretations than any others including those to which they are related by voice? One possibility is that these sentences are slightly anomalous, and one might even suppose that they are meaningless. Yet the answer, suggested by the subjects' actual classifications, is that they have more *possible* interpretations. This extra and unforeseen ambiguity is due to two factors.

First, the word "some" in a predicate with a negative verb-phrase has an additional ambiguity. So far, we have been considering the word pronounced [sʌm] which tends to occur with count nouns. There is another word, pronounced [sm̩], which tends to occur with mass nouns (cf. Bolinger, 1960). The difference between these two words is important in negative sentences: compare "John doesn't like some ([sʌm]) people" with "John doesn't want some ([sm̩]) butter." The latter is logically equivalent to "John wants no butter whatsoever" but the former is not equivalent to "John likes no people whatsoever." A written sentence like

"John doesn't want some pens" may thus receive any of three interpretations: John wants no pens whatsoever, John doesn't want some particular pens, or John doesn't want some pens or other (i.e. he doesn't want just any or every pen).

Second, sentences with a negative verb-phrase and a realization of the universal quantifier in their subject are ambiguous. For example, "All women do not lie" may mean either that lies are not told by *any* woman (no woman lies) or that lies are not told by *every* woman (not all women lie). This ambiguity about the scope of negation occurs whether the verb is transitive or intransitive, whether it is active or passive, and whether the quantifier is "every," "any" or "all." It even occurs in attributive descriptions like "All the square is not red" (Wason, personal communication).

Combining the two sources of ambiguity yields six possible interpretations for sentences (5) and (7). They may both receive any of the following interpretations, logically equivalent to those predicted for sentences (5), (6), (7), (8), (10) and (12):

(5) Some toy or other is not loved by any child.

(6) Some toy in particular is not loved by any child.

(10) No toy whatsoever is loved by any child.

(8) Some toy or other is not loved by every child.

(12) Some toy in particular is not loved by every child.

(7) No toy whatsoever is loved by every child.

It is a moot point whether these interpretations are possible for the voice-correlated sentences (6) and (8):

(6) Some toy is not loved by any child.

(8) Some child does not love any toy.

On balance, it would seem that they are possible: "Some ([sm]) child *isn't* sick" may be interpreted as "No child is sick," and "John isn't loved by any child" may be interpreted as "John isn't loved by *just* any (every) child." Hence, theoretically it would seem that sentences (5), (6), (7) and (8) are all ambiguous in the same way.

This startling consequence is not borne out in actual performance. Sentences (6) and (8) are interpreted very much as though they were unaffected by the extra sources of ambiguity and are clearly differentiated one from the other. Word order is crucial: "Some" at the beginning of a negative sentence is even less likely to elicit the interpretation "none" than the interpretation "some or other"; "any" at the end of a negative sentence is more likely to elicit the interpretation "none" than the interpretation "not all." Figure 2 shows that the most likely interpretations for sentences (5) and (7) are their respective privileged and unprivileged interpretations, though the other possibilities do all occur. This suggests that sentences like "Any man does not know some woman" tend to receive interpretations consistent with "Some woman is not known by *any* man," whereas "Any woman is not known by some man" tends to receive interpretations consistent with "Some woman is not known by *every* man." This is an effect on meaning which occurs in performance and which is due to voice and *not* to word-order changes brought about by voice (cf.

Johnson-Laird, 1968*b*). It seems that the scope of negation tends to be minimized with respect to the "actor," but maximized with respect to the "acted-upon."

There are a number of possible explanations for the differences in the classification times. It is possible that they are due to the pattern recognition aspects of the task, although the majority of subjects adopted no such explicit strategy. An informal study in which four subjects classified the matrices according to specified *patterns* showed that the average times were 8·0 sec. for "at least one column of "o's"" and 9·4 sec. for "at least one 'I' in every row." The average times for the sentences were all over 40 sec. It therefore seems unlikely that the pattern recognition aspects of the task were important. Since there was no reliable interaction between polarity and word order, it is possible that these variables had their effects on the classification times for the predicted reasons. There is, however, a strong likelihood that it was the ambiguity of the sentences which was crucial. Recent studies (MacKay, 1966; MacKay and Bever, 1967) have indicated that the more ambiguous the sentence or phrase, the longer it takes to interpret or to complete.

It seems that the negation of falsity is not psychologically equivalent to truth. The reliable bias towards classifying matrices as falsely described indicates that when subjects were in doubt, they chose falsity as the "safer" classification. When neither the privileged nor the unprivileged classification was made, there was a tendency to go outside the quantificational calculus, e.g. one and only one man does not know any woman. But, often this could be explained as a zealous, if naïve, attempt to distinguish some *or other* from some *in particular*.

The fact that both privileged and unprivileged interpretations occurred demonstrates both surface and deep-structure processing of the sentences. Clark and Begun (1968) have shown both levels of processing operating in tasks where subjects had to detect or correct semantic anomalies. Katz and Postal (1964) proposed that, for models of competence, all semantic interpretation is performed on deep structure, i.e. grammatical transformations do not change meaning. It might therefore be supposed that, in actual performance, the listener needs only to extract the deep structure in order to understand the sentence. The assignment of surface structure would occur only in so far as it was necessary for the assignment of deep structure and, more importantly, it would be held only temporarily in immediate memory. This is consistent with the fact that individuals remember sense not syntax. But it is this view which the present experiment casts some doubt upon: left-to-right processing *is* important because it biases the interpretative machinery. Surface structure provides clues to deep structure, but these clues—and in particular word order—may point to only one interpretation of an ambiguous sentence. There is one difficulty with this argument, due to the lack of an account of the semantics of quantifiers. It is possible that the particular ways in which quantifiers are introduced into deep structures, or changed when deep structures are transformed into surface structures, would yield radical differences in the deep structures of voice-correlated sentences. It is hoped that this study of performance has demonstrated the need for a linguistic account of the semantics of quantifiers.

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PROVING A DISJUNCTIVE RULE

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This experiment was designed to determine whether individuals reason correctly about disjunctive rules. The task consisted in the selection of appropriate instances either to prove, or to disprove, a given disjunctive rule. When the first component of the rule was negated, i.e. when the rule was logically equivalent to implication ($\bar{p}$ v q), the selection of appropriate instances was significantly more difficult than when the first component was not negated. The majority of subjects, however, revealed patterns of reasoning which were unstable and labile. The results are discussed in relation to those of previous experiments in which subjects had to reason about conditional sentences.

Introduction

Previous experiments (Wason, 1968) have shown that very few intelligent adults reason adequately about conditional sentences of the form, "if p then q". For example, given a sentence like, "if there is a D on one side of a card, then there is a 3 on the other side", together with four cards bearing respectively D, 3, B, and 7, very few subjects recognized that the 7 was relevant to finding out whether the sentence was true or false. The form of the words, "if p then q," however, is only one way of expressing the concept of implication ($p \supset q$). It can be expressed in different words using different logical constants, e.g. "there isn't p without q"—($p\bar{q}$), or in disjunctive form, "either there isn't p or there is q, or both" ($\bar{p}$ v q). Is the difficulty of "if p then q" inherent in the concept of implication, or is it due to the words used to express the concept? The present investigation attempts to answer this question by testing the disjunctive form. It should be noted that the appropriate disjunctive form is *inclusive* rather then *exclusive*, i.e. the sentence is true when $\bar{p}$ is true, when q is true, and when both $\bar{p}$ and q are true; it is false only when p and $\bar{q}$ are true. In the present investigation, however, the inclusive inter-pretation was assumed and the words, "or both," were omitted from the material.

Since the propositional calculus counts the following combinations of the conditional as true, pq, $\bar{p}q$, $\bar{p}\bar{q}$, and only the one combination, $p\bar{q}$, as false, it can readily be seen that ($p \supset q$) and ($\bar{p}$ v q) have the same truth table. But although the conditional and the disjunctive form of implication have the same truth table, they have a different meaning. Previous research (Wason, 1968; Johnson-Laird and Tagart, unpublished) has shown that the expression, "if p then q," is construed as true only when both p and q are true. Thus the expression, "either there

isn't p or there is q," would appear to be a more accurate description of the truth table.

It is known that the errors made in evaluating negative sentences are considerably greater than those made in evaluating affirmative sentences (Wason, 1959, 1961). Hence in the present investigation the disjunctive form of implication ($\bar{p}$ v q) was compared with the unnegated disjunction (p v q). (p v q) provides a base line against which the difficulty of ($\bar{p}$ v q) may be measured. Even though the present task is effectively binary, i.e. $\bar{p}$ takes only one value, it was predicted that more errors would occur under the ($\bar{p}$ v q) condition than under the (p v q) condition.

In the previous experiments (Wason, 1968), the subjects' task was to select all those values necessary to determine whether a given conditional sentence was true or false. In the present investigation the subject was either told that the sentence was true, or that it was false, and then instructed to select values in order to prove this. Knowledge that the sentence is true or false does not logically affect the correct response—it is the same in both cases. When the sentence is in the form ($\bar{p}$ v q) the correct response is to select p and $\bar{q}$; when it is in the form (p v q) the correct response is to select $\bar{p}$ and $\bar{q}$. However, when the sentence is known to be true it was considered more likely that the subject would make an erroneous "matching response," i.e. he would select just those values mentioned in the sentence. Similar results have been noted in the attainment of disjunctive concepts by Bruner *et al.* (1956), and in a classification task involving disjunctive sentences by Johnson-Laird and Tagart (unpublished). It was considered, in addition, that this effect would be more pronounced with ($\bar{p}$ v q) than with (p v q) since it was anticipated that the latter would be too easy to reflect the effect. Hence the following interaction was predicted: the difference between performance in the "true" and "false" conditions would be larger for ($\bar{p}$ v q) than for (p v q).

The test sentences which were used in this experiment referred to all the stimuli which were presented simultaneously to the subject, i.e. they were in the form, "Every card hasn't a P on one side, or it has a Q on the other side," or "Every card has a P on one side, or it has a Q on the other side." This allowed another factor in reasoning to be examined. There is an asymmetry in proving the rule true and proving it false. Since under the true condition every correct stimulus was consistent with the rule, and under the false condition every correct stimulus was inconsistent with the rule, every correct stimulus has to be inspected to prove the rule true but a single correct stimulus is sufficient to prove the rule false.

Method

Design

There were four basic experimental conditions: ($\bar{p}$ v q)T, i.e. true, ($\bar{p}$ v $\bar{q}$)F, i.e. false, (p v q)T and (p v q)F. The subjects acted as their own controls and were presented with these conditions once, ordered in a different one of each of the 4! permutations.

Subjects

Twenty-four volunteer undergraduate subjects of University College London were individually tested.

Materials

The following four types of lexical material were constructed:

(A) Every card has a number which is Roman on one side, or it has a letter which is capital on the other side.

(B) Every card has a rectangle which is vertical on one side, or it has an angle which is acute on the other side.

(C) Every card has a square which is black on one side, or it has a line which is crooked on the other side.

(D) Every card has a number which is even on one side, or it has a letter which is a vowel on the other side.

Each of these four types of material was, in addition, negated in its first clause so that it corresponded to ($\bar{p}$ v q), e.g. "Every card has a number which isn't Roman on one side, or it has a letter which is capital on the other side."

These eight sentences were typed on separate cards (5 × 3 in.). It will be noted that the sentences are not strictly binary, but the stimuli used in conjunction with them made them binary. For each of the sentences eight stimulus cards were constructed consisting of exemplars of p or $\bar{p}$ on one side and q or $\bar{q}$ on the other side. The stimuli used for the four sets of lexical material were as follows:

(A) Roman numbers, Arabic numbers; capital letters, lower case letters.

(B) Vertical rectangles, horizontal rectangles; acute angles, obtuse angles.

(C) Black squares, white squares; crooked lines, straight lines.

(D) Even numbers, odd numbers; vowels, consonants.

Table I shows the logical values on the stimulus cards associated with each of the four experimental conditions. The values given first of all are those on the front of the cards, and those given second in brackets are those on the back of the cards.

TABLE I

The logical values on the stimulus cards and the number of cards associated with each experimental condition

	p(p)	q(p)	p($\bar{q}$)	$\bar{q}$(p)	$\bar{p}$(q)	q($\bar{p}$)	$\bar{p}$($\bar{q}$)	$\bar{q}$($\bar{p}$)	
(p v q)*T*	1	1	1	2	2	1	—	—	8
(p v q)*F*	1	1	1	—	—	1	2	2	8
($\bar{p}$ v q)*T*	2	1	—	—	1	1	1	2	8
($\bar{p}$ v q)*F*	—	1	2	2	1	1	1	—	8

The explanation for the choice of the particular set of values for each condition is as follows. A necessary constraint is that each of the values p, $\bar{p}$, q, $\bar{q}$, should appear twice on the front of the cards, making a total of eight cards. Thus in the (p v q)T condition it is necessary for $\bar{q}$(p) and $\bar{p}$(q) to occur twice because inclusion of $\bar{p}$($\bar{q}$) or $\bar{q}$($\bar{p}$) would render the rule false. A second constraint is that under the false conditions all four correct cards should render the rule false. Thus for the (p v q)F condition it is necessary that $\bar{q}$(p) and $\bar{p}$(q) be omitted. It will readily be seen that similar principles apply to the stimuli in the ($\bar{p}$ v q)T and ($\bar{p}$ v q)F conditions. Inspection of the table also shows that the correct stimulus cards are those which are doubly represented in each condition.

Procedure

The first 12 subjects were presented with the lexical material in the order A, B, C, D, and the second 12 subjects in the order D, C, B, A. Under each of the four experimental

conditions the appropriate sentence was presented and the eight corresponding stimulus cards placed on the desk in front of the subject in a random array. It was explained to the subject that each sentence was a rule. Each condition had three phases:

(1) The subject was told that the rule was true (false), and that he had to pick out the four cards which he needed to turn over in order to prove this. He was told that there were only four cards which it was necessary to inspect. The subject was given as much time as he needed to think, and then selected four cards.

(2) The experimenter slowly turned over the cards selected by the subject, one at a time, and the subject was asked to tell him as soon as he was sure that the rule had been proved true (false).

(3) The subject was asked why he had not chosen the four remaining cards.

Results

Each subject was given a score from 0 to 4 for each condition according to the number of correct cards which he had selected. The mean scores were as follows: $(p \vee q)T = 2{\cdot}9$, $(p \vee q)F = 3{\cdot}3$, $(\bar{p} \vee q)T = 2{\cdot}6$, $(\bar{p} \vee q)F = 2{\cdot}7$. The prediction that the negative conditions would be more difficult than the affirmative ones was confirmed ($P = 0{\cdot}04$, Wilcoxon test, one-tailed). There was no significant difference between the true and false conditions and the predicted interaction was not confirmed.

Table II shows the frequency of the selection of the logical values for each condition.

TABLE II

Frequency of the selection of logical values for each condition

	$\bar{p}\bar{q}$	pq	$\bar{p}q$	$p\bar{q}$	Others	Totals
$(p \vee q)T$	17	6	0	0	1	24
$(p \vee q)F$	19	3	2	0	0	24
$(\bar{p} \vee q)T$	3	2	6	13	0	24
$(\bar{p} \vee q)F$	4	2	4	12	2	24

The frequencies in Table II, except those classified under "others," refer to the four selected cards, two for each value specified, e.g. the frequency of 17, under $(p \vee q)T$ for $\bar{p}\bar{q}$, means that in 17 cases two values of $\bar{p}$ and two of $\bar{q}$ were selected. The selections classified under "others" consisted of two cases of subjects selecting one value of each of the four values, p, $\bar{p}$, q, $\bar{q}$, and in one case of selecting two values of q and two of $\bar{q}$. The value of χ^2 was computed independently for each condition and was significant in each case. It will be noted that, under all conditions, a correct selection was made more frequently than any other selection. Ignoring truth and falsity, $\bar{p}\bar{q}$ was selected 36 times out of 48 for $(p \vee q)$, and $p\bar{q}$ was selected 25 times out of 48 for $(\bar{p} \vee q)$. Only four out of the 24 subjects were correct for all four conditions.

The number of cards which the subjects needed to turn over in phase 2 before making a decision that the rule was true, or false, was computed independently for

the *T* and *F* conditions. Only correctly selected cards were included because under the *F* conditions incorrectly selected cards do not falsify the rule. Each subject had two trials in the T condition and two in the *F* condition, and since a subject may make an incorrect selection on one or both trials, mean values over the two were computed. Table III shows the number of subjects associated with each of these mean values. The correct number of cards to turn over in order to verify the rule is four, and the correct number to falsify it is one. Hence it will be noted that most subjects made a good approximation to the rational strategy.

TABLE III

Mean number of cards turned over to prove that the rule is true or false

Mean number of cards turned over	Number of subjects True	False
4·0	11	3
3·0	1	2
2·5	1	2
2·0	2	0
1·5	0	1
1·0	3	13
Subjects making incorrect selections on both trials	6	3

At the end of the experiment an informal test was made of whether the subjects had been reasoning by inclusive or exclusive disjunction. The remaining cards were turned over by the experimenter to see whether a card which had *both* the values mentioned in the rule was counted by the subject as consistent or inconsistent with the rule. In fact only three subjects gave evidence of reasoning exclusively.

Discussion

It is evident that the element of negation, which is inevitably present in the logical structure of implication, does, to some extent, contribute towards its difficulty. 52·1 per cent of the subjects reason correctly when implication is expressed in disjunctive form, compared with 75 per cent when disjunction is not negated.

It is of interest to observe the types of error made in this experiment. Under the (p v q) conditions matching responses (mirror images of the correct selections) accounted easily for the majority of errors—9 out of 12. On the other hand, under the ($\bar{p}$ v q) conditions such responses accounted for only 10 out of 21 errors. It seems likely that the negative component induces a confusion which manifests itself in a greater variety of erroneous responses. There were 12 matching responses under the *T* conditions as opposed to 7 under the *F* conditions—a trend which is consistent with the predicted interaction between the truth of a rule and its

logical form. Contrary to the prediction, however, the difference between the matching responses for the *T* and *F* conditions was more pronounced for (p v q) than for ($\bar{p}$ v q).

When correct selections of cards had been made the reasons given for rejecting the remaining cards tended to reveal an appreciation of disjunction. "Those can be seen to prove at least half the rule." "Those four satisfy the conditions; so I do not need to turn them." When incorrect selections had been made the reasons reflect the erroneous matching principle and, in at least one case, a conflation between disjunction and conjunction. "Those are straight lines and white squares. The rule requires crooked lines and black squares." "Those are already incorrect on one side. I don't know what is on the other side. You just can't do it with four cards." "I took the four which had an even number and a vowel so that if it had the other on the back it would prove the rule true."

A very interesting effect due to the truth of the rule was observed. Four subjects matched on both the true conditions, but three of them made correct selections on both the false conditions. When the rule was true the following were the reasons for rejecting the remaining cards. "You are trying to prove the rule is true and these might disprove it." "Those others are not what is stated in the rule and the rule is true." "I chose the true ones. No matter what is on the other side, these would proved the rule." When the rule was false, these subjects made the correct selections for the wrong reasons. "If I am trying to prove the rule false, then it is obvious you wouldn't choose the ones which prove it correct." "Those others are what is stated in the rule and the rule is supposed to be false." "Those others are half of the rule which I'm trying to disprove."

The results of this experiment, when compared with those of Wason (1968), show that expressing implication as the disjunction, "either not-p or q", makes it easier to grasp than expressing it as the conditional, "if p then q." Just over half the subjects reasoned efficiently about the disjunctive expression, but in the previous experiments only 16·7 per cent of the subjects selected $\bar{q}$. Thus the verbal guise, or description, of a rule has a marked effect on the way it is understood.

The most striking differences, however, are not just quantitative; they are qualitative. When the subjects are given the rule in the form "if p then q," they tend to go wrong in a decisive way. And little that one can do will evidently prevent them falling into error. No such feeling of finality attached to the responses in the present experiment. Sixteen out of the 24 subjects spontaneously doubted that some of their selections were correct. These doubts were expressed on 17 incorrect selections and on 4 correct ones. "I should have chosen the capitals instead of the lower case letters. They would prove the rule false" (selection right, comment wrong). "I was working on the wrong assumption. I could have done it more easily if I had chosen the other four" (selection right, comment wrong). "My mind went blank. Please let me think again" (selection wrong, comment right).

The labile quality of this kind of reasoning seemed to be associated with both affirmatively and negatively expressed disjunctions. One consequence was that the subjects were ready both to admit error and to gain spontaneous insight into their mistakes after they had made them. Very frequently the act of committal to a

choice immediately evoked misgivings. It is as if the "either . . . or" expression itself creates uncertainty. It breaks up the "direction" which seems to be strongly imposed by the conditional, "if . . . then," sentence. With a conditional the individual is likely to be confident but wrong; with a disjunction he is more likely to be unconfident but right. The meaning of a conditional gives no hint of the negation or falsity which underlies its logic. The disjunctive expression makes this element explicit, but this seems to weaken the grounds upon which any inference can be made. In conclusion, a tentative answer may be given to the question posed in the Introduction. There would seem to be inherent difficulties in the concept of implication, but the words in which it is expressed also effect both its ease of understanding and the manner in which it is construed.

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COGNITIVE CATEGORIES AND MEMORY SPAN

III. EFFECTS OF SIMILARITY ON RECALL

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The effects of similarity on short-term recall are investigated, by presenting lists of six consonants followed by short lists of consonants and more or less remote cognitive categories, i.e. digits, tones or spatial positions. Some evidence for release from interference, due to dissimilarity of interfering material, is found with consonants and tones, while almost complete release is shown when consonants are followed by spatial positions. Control experiments are included to account for alternative explanations, such as relative difficulty of the various categories, and different memorizing strategies for items, made up of many as opposed to two alternatives. The latter factor appeared to be of particular importance.

Introduction

A very critical issue in the controversy between decay and interference theory to explain forgetting in short-term memory (STM) concerns the effect of similarity on short term recall (Broadbent, 1963; Melton, 1963). It has been repeatedly shown that STM is sensitive to acoustic similarity (Conrad and Hull, 1964) and *not* to semantic similarity, as is usually found in verbal learning (Baddeley, 1966) but this has not been considered as clear evidence for interference theory. Other studies have shown effects of proactive and retroactive inhibition in STM (Sanders, 1961; Keppel and Underwood, 1962; Keppel, 1965; Wickelgren, 1965). The problem here is that a memory span type of experiment is not a pure measure of the contents of the hypothetical STM mechanism, but rather reflects a mixture of short- and more long-term effects. Some parts of a list can resist forgetting much better than other parts (Sanders, 1961) and a list, which is repeated within a test session, gradually shows better recall than non-repeated lists (Hebb, 1961). Therefore, the effects of proactive and retroactive inhibition may be solely due to the long term components.

A strong test between interference and decay theory may be obtained by presenting lists, which consist of successive sublists from more or less remote cognitive categories. An interference theory would not predict substantial forgetting as long as the sublists are both within their respective memory spans and when the categories are remote in the sense that there are almost no mutual associative bonds between elements. A decay theory, on the other hand, would predict the same amount of forgetting, whatever categories are used, since it is assumed that

forgetting is mainly dependent on the time-lag between presentation and recall. So the decay theory is supported by data which suggest a constant attention factor (Mackworth, 1964) or a constant STM capacity in the case of recall of multi-dimensional signals (Harrison, 1967).

In an earlier paper (Sanders and Schroots, 1968*b*), an outline was given of a method to determine mutual differences between cognitive categories in an STM setting. It appeared that digits are more related to consonants than tones are, and that spatial positions are most remote from consonants. In the experiments that follow, it will be investigated how far successive sublists of these categories show forgetting in comparison with lists from the same class.

Method and Procedure

The general plan of the experiments was to present lists of six consonants (6C), supplemented with a varying number of items from another category—i.e. digits, tones or spatial positions—or supplemented with more consonants. In the following the supplements will be indicated by RE (remaining elements) when the items are from another category and by RC (remaining consonants) when they are also consonants. The list length of RE and RC ranged from 2 to 6 in the case of digits and tones and from 1 to 5 in the case of spatial positions. Hence, the total list length (6C + RE, or 6C + RC) was minimally 8 and maximally 12 when RE consisted of digits or tones, and ranged from 7 to 11 when RE consisted of visual spatial positions.

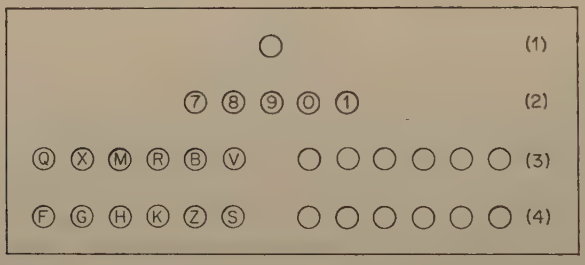

FIGURE 1. A schematic sketch of the visual display (Group III).

For each total list length in a particular condition, ten lists were constructed. All lists were randomly presented at a presentation rate of two signals per second. Ample time was given for recall, which took place after presentation of each list. Subjects were informed each time about length and structure of the next list, i.e. 6C + RE or 6C + RC. The consonants were always from the set (B, F, G, H, K, M, Q, R, S, V, X, Z), which was on continuous display. No consonant was repeated within a list.

Items were only scored as correct when reproduced in the correct serial position. Subjects were informed about this and were instructed to leave open spaces or to guess on locations where items had been forgotten. Three groups of freshmen from the university of Utrecht took part in the experiment. Each group recalled 100 lists, divided in 50 experimental lists (6C + RE) and 50 control lists (6C + RC). Group I consisted of 8 subjects and had digits as RE. The digits were taken from the set 0 through 9, and no digit occurred more than once in a list. Group II contained 7 subjects and had tones as RE. To avoid coding problems, only two tones were used, i.e. a high- and a low-pitched tone (262 and 524 Hz). Recall of both groups was by writing on a response sheet. The tones were symbolized by an upward (↑) and a downward (↓) arrow. Both groups received auditory presentation. All lists were recorded in advance on magnetic tape. All subjects in each group were tested at the

same time. Group III consisted of 7 subjects and was given visual spatial positions as RE. A visual presentation technique was needed therefore, the details of which are described elsewhere (Sanders and Schroots 1968*b*; van Doorne and Sanders, 1968). It may suffice to show a schematic sketch of the display, on which the items were successively presented (Fig. 1). The number of items to appear in a list was indicated before presentation by a 1-sec. flash of the appropriate light in row 2 of Figure 1. In case of a 6C + RC list the top row light flashed at the same time, but not in the case of 6C + RE. Consonants were mounted on top of the lights at the left side of the rows 3 and 4, and were only visible during presentation. The lights themselves were mounted on reaction keys. Therefore, a 6C + RE list consisted of a succession of flashes: the first six flashes made consonants appear, the remainder only lights, which had to be retained on the basis of spatial position. The consonants were reproduced by writing them down and the lights by pressing the succession of positions. Each subject was tested individually. The number of correct items was calculated for each subject averaging over the ten lists of the same length and composition. These numbers were transformed to percentage correct recall of the 6C part, and separately, of the RE or RC part. In this way both parts of the list were equally weighted. This exposition outlines only the methods and procedure of the basic experiments. It proved to be necessary to carry out an additional number of control studies, which are described separately at appropriate places.

Consonants and Digits

Results

Mean percentages of correct recall of Group 1 are presented in Table I. An analysis of variance, followed by *post hoc* comparisons of individual means

TABLE I

Mean percentages of correct recall (Group 1, main experiment)

List composition	List length									
	6 + 2		6 + 3		6 + 4		6 + 5		6 + 6	
Consonants + digits	68·4	88·5	56·5	75·3	61·0	53·7	53·0	38·4	54·3	39·4
Consonants + consonants	65·8	60·5	59·0	52·0	65·0	42·5	48·3	30·3	46·0	27·5

according to the Newman-Keuls procedure showed significant effects of list composition (factor A: $F = 39{\cdot}16$; *d.f.* 1, 19), list length (factor B: $F = 54{\cdot}34$; *d.f.* 4, 91) and subjects (factor D: $F = 24{\cdot}95$; *d.f.* 7, 91). No differences appeared between parts of the lists (factor C), but a significant A × C interaction ($F = 26{\cdot}63$; *d.f.* 1, 91) indicated that performance on either part of the list is related to list composition. The better recall of successive sublists is only reflected in a difference between RE and RC. Finally, a significant C × D interaction ($F = 19{,}36$; *d.f.* 7, 91) showed that performance on 6C vs. RE RC is affected by individual preferences for points of lists.

Control experiment and discussion

It would be premature to explain these results as due to categorical differences, in view of the possibility that the effect is a matter of relative difficulty to memorize consonants and digits (e.g. Mackworth 1963; Sanders and Schroots 1968*a*). This

alternative requires a control, in which digits are supplemented with consonants. Method and procedure were analogous and ten student subjects took part. The results are in Table II.

TABLE II

Mean percentages of correct recall (control experiment)

List composition	List length 6 + 2		6 + 3		6 + 4		6 + 5		6 + 6	
Digits + consonants	79·5	95·5	66·0	90·0	56·0	79·0	51·5	65·0	47·5	65·0
Digits + digits	78·0	91·5	72·0	81·0	58·5	57·5	53·0	48·5	50·5	44·0

Analysis of variance showed significant effects of list composition (factor A), list length (factor B) and subjects (factor D). ($F = 6{\cdot}57$; *d.f.* 1, 9; $F = 11{\cdot}96$; *d.f.* 4·72; $F = 40{\cdot}7$; *d.f.* 9, 12.) Also, a significant A × C interaction was found ($F = 9{\cdot}36$; *d.f.* 1, 9), which was due to better recall of consonants. Therefore, the results are at odds with those of Group 1, which favours the hypothesis of release of interference. The effect is rather slight however and only reflected in better recall of RE. This may not surprise, since consonants and digits are still quite related categories. Furthermore it is still questionable whether the better recall of RE is specifically due to categorical differences. Any structural change—e.g. a short extra time lag—within a list, may provide an anchor, resulting in slightly better recall (Yntema, Worencraft and Klem, 1964).

Nevertheless, the results provide a baseline. When more remote categories are employed as successive sublists more gain should be expected than found here.

Consonants and Tones

Results

Mean percentages of correct recall of Group II are found in Table III. 6C + RE lists are much better recalled than 6C + RC lists (factor A: $F = 234{\cdot}73$; *d.f.* 1, 78)

TABLE III

Mean percentages of correct recall (Group II, main experiment)

List composition	List length 6 + 2		6 + 3		6 + 4		6 + 5		6 + 6	
Consonants + tones	81·2	93·0	64·8	86·7	59·8	92·5	59·5	80·8	51·8	85·4
Consonants + consonants	62·2	75·0	54·5	59·0	43·0	50·0	45·0	37·4	34·3	34·0

and also list length and subjects differ significantly (factor B: $F=28{\cdot}94$; *d.f.* 4·78; factor D: $F = 25{\cdot}09$; *d.f.* 6, 78). Significant A × B and A × B × C interactions ($F = 3{\cdot}45$; *d.f.* 4, 78; $F = 3{\cdot}29$; *d.f.* 4, 78) indicated that effect of list length is related to list composition and to part of the list. The percentage of correctly

recalled tones did not significantly decline as a function of list length, while consonants were better retained in 6C + RE lists (A × C: $F = 38{\cdot}38$; *d.f.* 1, 78).

Discussion and control experiments

Successive sublists have a large positive effect in this condition. Recall of tones remains about equally high as a function of list length, and also consonants are better recalled, when followed by tones; although there is still forgetting at long lists. However, as in the case of consonants and digits, the control is required for effects of relative difficulty. This seems even more urgent since recall of tones may be highly affected by guessing. It was not justified to correct the data of Table III for guessing with the usual formula, requiring randomly distributed errors, since analysis of occurrence of errors in lists of tones showed much more clusters than could be expected on the basis of the probability distribution. However it can be assumed that guessing exerts some effect. A control experiment was carried out therefore, in which lists of tones (8–12), lists of consonants (8–12) and six tones, supplemented with consonants (2–6) were presented. Each condition and list length had six replications making a total of 90 lists. Ten students served as subjects, and the results are presented in Table IV. They show that tones are

TABLE IV

Mean percentages of correct recall (control experiment)

List composition	List length 6 + 2		6 + 3		6 + 4		6 + 5		6 + 6	
Tones + tones	89·6	73·6	84·4	62·8	77·9	52·4	73·6	50·8	67·8	46·8
Tones + consonants	81·7	94·9	80·9	94·8	81·4	90·9	68·9	79·8	71·2	67·6
Consonants + consonants	56·8	62·5	47·6	57·6	52·4	50·3	45·4	47·4	36·3	37·6

better recalled than consonants but that best performance is obtained with successive sublists (factor A: $F = 95{\cdot}86$; *d.f.* 2, 18). Hence, better recall of successive sublists cannot be explained by relative difficulty or by guessing, although the data of Table IV show a that part of the improvement can be ascribed to such factors.

An interesting difference between recall of consonants and tones is that better recall of tones primarily occurs in the first part of the list (A × C: $F = 15{\cdot}52$;

TABLE V

Mean percentages of correctly recalled tones as a function of serial position

List length	Serial position 1	2	3	4	5	6	7	8	9	10	11	12
8	93	88	90	90	85	75	68	75				
9	93	90	83	82	82	62	65	57	65			
10	87	82	73	70	75	68	47	45	45	57		
11	68	82	70	75	68	50	52	35	42	58	68	
12	85	67	65	65	63	40	27	30	30	47	55	77

d.f. 9, 72). Analysis of recall as a function of serial position (Table V), shows a consistent high level of recall at the first five or six items, followed by a drop. The recency effect, which is generally found in recall of consonants, only appears at lists of length 11 and 12. Thus, performance in recall of tones, and perhaps in all two-alternatives sequences seems to be better on the first part of the list. It is not improbable that most guessing is found in the second part. At least, the results suggest a difference in memorizing strategy between lists of many and two alternatives.

Control experiment 2

Possible differences in memorizing strategy constitute another alternative explanation of the data of Table III and IV. The test included again three conditions: 6C + RE, 6C + RC and 6C + RE_1, where RE_1 indicates consonants from a set of two alternatives. List lengths (8–12) and conditions had six replications, making a total of 90 lists. Seven students served as subjects. The results are in Table VI. The significant effects were list composition (factor A: $F = 50{\cdot}79$;

TABLE VI

Mean percentages of correctly recalled items (control experiment)

List composition	List length 6 + 2		6 + 3		6 + 4		6 + 5		6 + 6	
Consonants + tones	76·2	87·0	76·2	76·0	69·0	82·0	67·5	72·8	65·0	70·7
Consonants + consonants (two alternatives)	60·7	97·5	47·7	86·7	45·1	86·2	31·7	76·2	40·0	68·2
Consonants + consonants	53·8	80·0	54·0	61·0	46·5	54·7	35·7	51·4	31·7	43·7

d.f. 2, 12), list length ($F = 37{\cdot}75$; *d.f.* 4, 10), subjects (factor D: $F = 15{\cdot}29$, *d.f.* 6, 104) and A × C ($F = 19{\cdot}23$; *d.f.* 2, 12). *Post hoc* comparisons showed no difference between 6C as a part of 6C + RC and 6C + RE_1, while 6C as a part of 6C + RE was better recalled. RE_1 and RE delivered about equal performance, both being superior to recall of RC.

Hence, the results show indeed an effect of changing the number of alternatives within a list: tones are as well recalled as consonants, consisting of 2 alternatives. On the other hand, 6C lists are only better recalled if they are followed by tones. This seems to be the only genuine effect of the categorical differences. It shows that, using consonants and tones, only partial release from interference is found.

Consonants and Spatial Positions

The results of Group III are presented in Table VII. The statistical analysis showed significant effects at factors A, B and D ($F = 27{\cdot}37$; *d.f.* 1, 6; $F = 21{\cdot}76$; *d.f.* 4, 76 and $F = 11{\cdot}23$; *d.f.* 6, 76) and at A × B ($F = 3{\cdot}26$; *d.f.* 4, 76) and A × C ($F = 7{\cdot}69$; *d.f.* 1, 76). The effect of list length was much stronger at 6C + RC. 6C as a part of RE did not significantly decline, when RE increased from one to five

items. RE declined only at list length 5. So, the results of Group III show that 6C + RE lists are much better recalled. It must be especially noted that both 6C and RE show no sizeable decrement, when list length increases. The decline of RE at list length 5 is in line with data on memory span for spatial positions (Sanders, 1968).

TABLE VII

Mean percentages of correctly recalled items (Group III, main experiment)

List composition	List length									
	6 + 1		6 + 2		6 + 3		6 + 4		6 + 5	
Consonants + positions	85·7	97·0	85·5	95·5	82·7	85·7	81·7	84·7	78·3	72·7
Consonants + consonants	85·7	76·0	74·8	65·6	67·7	45·3	62·7	42·7	60·1	34·6

It does not appear necessary to carry out control studies as was done previously. First, spatial positions have a lower span than consonants (Sanders, 1968), so that the argument of relative difficulty is not valid. Second, the number of alternative consonants and positions were equal. It seems justified, therefore, to ascribe the almost complete release from interference to the very marked categorical differences. When 6C + RE had a length of 10, 42·9 per cent of the lists were correctly recalled, making an average span of about 10 items, instead of the magical number seven (Miller, 1956). It may be that this large effect is related to different coding—i.e. acoustic with consonants and visual with spatial positions—reducing the mutual interference. Still, there is no complete additivity of memory span, since both RE and 6C may be slightly below the level that is found when they are measured alone. This may constitute a minor decay factor.

General Discussion

The results, taken together, show considerable evidence for improved immediate recall, when lists are made up of successive sublists from most remote categories. The effect is small and questionable at consonants and digits, more clear with consonants and tones, and approaches additivity of memory spans when consonants are followed by spatial positions. This is not in line with Mackworth's theory of immediate recall, as determined by a constant attention factor (Mackworth 1964) or with Harrison's suggestion of a constant STM capacity when multidimensional signals are to be recalled (Harrison, 1967). The results with multidimensional signals may be due to order of recall. It is probable that when recall is done per item—i.e. all attributes of one item followed by those of the next one—much less correct recall is obtained than when subjects recall categorically—i.e. one attribute of all items followed by the next attribute (Sanders and Schroots, 1968*b*). The latter strategy might enable subjects to recall all 16 values, when four items with four attributes each are presented, as was found on occasions by Harrison.

The data are consistent with a conception of STM in terms of short term activation of pre-existing units and associative bonds in a dictionary store, where

materials from more remote classes are less mutually interfering. A decay theory might still be correct, if the assumption of single channel transmission of items does not hold when remote categories are involved. If it is possible to rehearse during memorizing of items of the other class, a decay theory would not predict much forgetting. This possibility suggests some interesting experiments, both on STM and more typical topics from single channel theory such as the psychological refractory period.

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HYPOTHESIS FORMATION AND PROBLEM COMPLEXITY

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The present study examines the relationship between hypothesis formation and the degree of complexity of the problem which has to be solved. Ninety subjects played a simple arithmetical game with the investigator. They were divided into two groups according to the type of problem given. The hypotheses formed by the subjects in the course of the game were classified under two headings; namely, H_{I}, trying to form a general and comprehensive strategy on the basis of an overall evaluation of the problem; and H_{II} based on a more detailed logical analysis and leading to a strategy with sub-goals. Hypotheses of the first type prevailed in the simpler versions of the problem, those of the second type were more frequent in the more complicated versions. If the number of alternative strategies is small, subjects are not forced to develop an abstract formulation of the strategies, and continue to operate with concrete strategies (e.g. I always take two chips). When there are many alternative strategies, subjects are forced to use more abstract analyses of the problem which appears to involve formulation of sub-goals. The consequences of both types of hypotheses for problem solving are discussed.

Introduction

Behaviour during problem solving appears to be controlled by plans (Miller, Galanter and Pribram, 1960) or hypotheses (Wason, 1960) or strategies (Bruner, Goodnow and Austin, 1956). In the process of problem solving a subject may select one of many possible procedures for analysing and processing information, which he has either obtained beforehand in the form of instructions or which he has acquired in the process of learning. Scientific inference from hypotheses and finding criteria of plausibility of hypotheses is of concern in current philosophy of science (Barker, 1957). Making inferences from hypotheses in problem solving, and its relationship to scientific thinking, has been discussed by Wason (1960) who finds that "even" intelligent adults do not readily adopt a scientific attitude to a novel problem" (Wason, 1967). These findings raise the question of whether a "non-scientific attitude" has any connection with the structure of the problem given. Broadly speaking, the present investigation focusses on the level of generality of hypotheses formed by the S. and, in particular, on the relationship between the hypotheses and the complexity of the problem.

Variants of the game of "NIM" game were used in the investigation.† "NIM" is a game for two and, according to game theory classification, it involves a zero sum

† A description of this game was given in a paper read at XVIII International Congress of Psychology in Moscow 1966 (Marková, 1966).

and complete information. It is played as follows. We have a pile of chips from which both players take alternately either one chip or several chips according to the rules fixed in advance; the player left with the last chip is the loser. This game, in another variation, was investigated by Peterson (1920) and Waters (1928).

The number of chips at the start is determined by the formula: $nx + 1$, where $n =$ positive integer, $x = k + 1$, and $k =$ maximum number of chips which may be taken. If both players know the optimal strategy, then the winner is always the player who plays second. Here optimal strategy is introduced only intuitively: each player is allowed to take at least one chip and not more than $x - 1 = k$ chips in each move; thus the second player can always complete the number of chips taken by the first player up to the number $x = k + 1$. Suppose, for example, that the total number of chips is 13. The rule specifies that, in each move, the player is allowed to take 1, 2, or 3 chips. The second player, if he plays the optimal strategy, must always complete the number of chips taken to a multiple of four.

Definitions

(1) *Hypothesis.* In our work we start from the assumption that, in theoretical problems, the subject formulates hypotheses as propositions (e.g. hypothesis: On Mars there is life), whereas in practical problem solving (as in the NIM game, where the subject is supposed to perform an action to reach his goal), the subject *transforms hypotheses into imperatives* in his verbalization and in his thinking. On the basis of this assumption, we may infer hypotheses from the statements expressed by subjects, e.g., the hypothesis "if I take an odd number of chips in each draw I shall win" is transformed by subject directly into a prescription for action, which is verbalized as "take an odd number of chips in each draw."

(2) *Set.* We shall call each ordered k-tuple of ordered pairs of digits, which might make up an actual game, a set. Thus,

$$((i_1, j_1) \ldots\ldots\ldots\ldots (i_k, j_k)).$$

$i_u =$ number of chips taken by the investigator in the uth round (one round is each ordered pair of draws of investigator and the subject, who is always the second player). And,

$j_u =$ number of chips taken by the subject in the uth round.

The digit k is determined in the following way:

k = the only x such that

$$\left(\sum_{m=1}^{x} i_m + \sum_{n=1}^{x} j_n = y \right).$$

(3) *Strategy.* Strategy is such prescription for action as enables some potential sets to be rejected and other sets realized. Each strategy therefore "cuts out" a certain definite sub-class of sets.

(4) *Problem.* The problem given to subject is to find a rule which determines the value of j_u in dependence on i_u with the result that one of the following sets arises:

$$\begin{array}{l}((i_{1k}, j_{1k}) \ldots\ldots\ldots\ldots (1, 0)) \\ ((i_{2k}, j_{2k}) \ldots\ldots\ldots\ldots (1, 0)) \\ \quad\vdots \qquad\qquad\qquad\quad \vdots \\ ((i_{nk}, j_{nk}) \ldots\ldots\ldots\ldots (1, 0)).\end{array}$$

This is a class of sets where i_u is arbitrary and j_u is determined by the rule prohibiting the investigator from making an optimum draw.

(5) *An optimum draw* by the investigator in the uth round of a game would be the draw by which he completes the number of chips so far taken to a multiple of n.

(6) *The optimum game* for the subject would be the game in which the subject completes the number of chips taken to a multiple of n in each round.

(7) *The optimum strategy* of the subject would be such a strategy which eliminates those games which are not the optimum games of the subject.

(8) *The solution* of the problem given to the subject would be finding the rules of the optimum strategy.

Strategies are formulated in the statements of subject's as prescriptions for action. A hypotheses or a sequence of hypotheses may be ascribed to each strategy. Each strategy may be connected with several different hypotheses. For example, let us have the strategy exemplified by use of the set: $\langle\langle 3,1\rangle\ \langle 1,3\rangle\ \langle 1,3\rangle\ \langle 1,0\rangle\rangle$. From this strategy, we cannot reliably infer which hypothesis the subject played—whether according to: to take only odd chips; the first draw one, others draws three; to complete the number of chips taken in one round to an even number; to complete the number of chips taken in one round to four; etc.

Method

Procedure

There were two series of experiments. Our assumption was that the complexity of the game depended on the total number of chips used in the game and also on the number of chips which each player could take in each draw. In each series there were five variants of the NIM game. Four of them were actually played by manual manipulation of chips, and one variant was considered only theoretically as a test of the strategy developed.

First series. The games were in the following sequence: G_3^{13}, G_4^{13}, G_5^{16}, G_6^{19} (actual play); G_7^{50} (theoretical variant).

G—stands for the game. The subscript denotes the total number of chips for both subjects which must be taken in each round if the second player plays perfectly (optimum strategy). The superscript denotes the total number of chips in the game.

Second series. The games were in the following sequence: G_6^{19}, G_5^{16}, G_4^{13}, G_3^{13} (actual play); G_7^{50} (theoretical variant).

The following rules were imposed. If after 20 sets or 60 min. the subject was unable to arrive at a hypothesis which determines the class of optimum sets, then the game was terminated. The investigator always started off the game, playing as well as the situation

allowed. For each subject the number of chips taken by the investigator at the beginning of each set was as follows.

G_3^{13}: 1 2 1 2 1, etc. G_5^{16}: 1 3 2 4 1 3 2 4, etc.

G_4^{13}: 1 3 2 1 3, etc. G_6^{19}: 1 3 5 2 4 1 3 5, etc.

This arrangement put all the subjects on equal footing, and reduced the number of sets.

The subjects were 90 children, 15 years old at Prague comprehensive schools. They were selected at random among pupils with the highest marks in maths. The subjects were tested individually in a room in the school. They were divided into two groups of 45 subjects equalized according to score on the Raven matrices. The scores gave an I.Q. range of −115−132. The first group had 23 boys and 22 girls, the second 22 boys and 23 girls.

Instructions for the first game were as follows. "Here is a pile of chips. The number of chips is 13 (first series)—19 (second series). We shall both be taking chips from this pile, one after another. I shall be the first. Both you and I can take 1 or 2 chips (first series)—1 to 5 chips (second series) from the pile. The one left with the last chip from the pile loses the game. Do you understand? The game will be repeated 20 times. I shall always start. While you play, I would like you to think aloud. Please say all that comes into your mind as you try to solve the problem. Try to say everything even if you think that it has nothing to do with the game. Do you understand my instructions?" Where the subject did not understand, the instructions were repeated in the same words.

Instructions for the second, third and fourth game were as follows:

"Now here is a pile of 13 (first series)—16 (second series) and we are allowed to take 1, 2 or 3 chips (first series)—1 to 4 chips (second series) from the pile. As before, we shall both be taking chips from this pile one after another and I shall always start first."

Instructions for the fifth (theoretical) game were as follows.

"Now imagine that there is a pile of 50 chips. In each draw you are allowed to take 1 to 6 chips. How would you manage to win each game if I again always start first?"

This theoretical variant was given at the end of each series only to the subjects who successfully solved the actual games.

Scoring criteria

We can distinguish logically two types of hypotheses:

Hypotheses of the first type, H

Definition	*Typical verbalization*
When (and only when) I carry out such and such sequence of moves, I shall win.	I shall carry out the following sequence of moves....

It is obvious that in hypotheses of this type the subject tries to determine simultaneously all the sets whose final round is $<1, 0.>$ As the H_I governs the whole action of the subject, it is necessary, if the hypothesis proves false, to substitute it with another, completely new hypothesis. Hypotheses H_I are based neither on the analysis of problem situation nor on the analysis of the goal.

Hypotheses of the second type, H_{II}. This is really a sequence of hypotheses. This sequence consists of individual hypotheses that determine individual sub-goals only. The last hypothesis is in terms of an actual draw.

Definition	*Typical verbalization*
(1) When (and only when) I reach the situation A_1 I shall win.	(1) I shall reach the situation A_1.
⋮	⋮
(2) When (and only when) I reach the situation A_m I shall reach the situation A_1.	(2) I shall reach the situation A_m.

The hypotheses formed by the subjects in the course of the games were classified into the two types distinguished above, i.e. hypotheses of the first type, H_{I}, trying to form a general and comprehensive strategy on the basis of an overall evaluation of the problem; and hypotheses of the second type, H_{II}, based on more detailed logical analysis and leading to a strategy with sub-goal aims.

Classification into these two categories was made on the basis of the transcription of actual utterances by subjects in the course of the experiment. Individual moves both of the subject and the investigator were also recorded, but classification of hypotheses was made only on the basis of the verbalizations. This appears to be valid (Markova, 1964). The first two or three sets of the first game were mostly devoid of any verbalization: statements of the subjects were here of the type "I do not know why I have made this move" or " I will try this or that for the time being": the strategy is chaotic and nothing can be extracted from it that would correspond with H_{I} or H_{II}. The stage of problem solving beginning, on average, with the third set of the first game is characterized by statements which can be classified as hypothese H_{I} or H_{II}.

If more than half of subjects verbalizations in the series of four actual games were of character H_{II}, his record sheet was classified as an overall example of H_{II}, and vice versa for H_{I}. These proportions were taken from, on average, five statement per first game and three statements per second, third and fourth game; but it was also true that subjects who used H_{I} produced more verbalizations than subjects who used H_{II}. Changeovers in hypothesis type could be observed in the data. For deciding that such changeovers had occurred, the same criterion, one-half, was applied to each of the four games. The changeovers are shown in Tables I and II.

TABLE I

First group

Changeovers	G_3	$G_3 \rightarrow G_4$	G_4	$G_4 \rightarrow G_5$	G_5	$G_5 \rightarrow G_6$	G_6
$H_{\text{I}} \rightarrow H_{\text{II}}$	0	0	1	2	2	1	0
$H_{\text{II}} \rightarrow H_{\text{I}}$	0	0	0	0	0	0	0
Σ	0	0	1	2	2	1	0

TABLE II

Second group

Changeovers	G_6	$G_6 \rightarrow G_5$	G_5	$G_5 \rightarrow G_4$	G_4	$G_4 \rightarrow G_3$	G_3
$H_{\text{I}} \rightarrow H_{\text{II}}$	4	1	0	0	0	0	0
$H_{\text{II}} \rightarrow H_{\text{I}}$	0	0	0	0	0	0	2
Σ	4	1	0	0	0	0	2

While not significant these results show that changes tended to be in the direction $H_{\text{I}} \rightarrow H_{\text{II}}$, and to be related to the complexity of the game rather than to the stage of the experiment.

Quantitative Results

Two questions are important. First, are the relative probabilities of hypotheses of types H_{I} and H_{II} different for the two experimental groups? Second, is there any connection between the type of hypothesis selected and the successful solution of the game?

The data relevant to the first question are contained in Table II, which shows the number of subjects in each group choosing each type of hypothesis. The relative probabilities of H_I and H_{II} may be said to be different in the groups if Table III is significantly non-homogeneous. The likelihood ratio test for homogenity in contingency tables is discussed by Wilks (1935). The test statistic is

$$\hat{I}(Y_1 : Y_2) = \log \frac{P(\text{data}/Y_1)}{P(\text{data}/Y_2)}$$

where Y_2 is the hypothesis of homogeneity, (i.e. $P(H_I/\text{group } 1) = P(H_I/\text{group } 2) = \alpha$, say) and Y_1 is the alternative hypothesis which permits $P(H_I)$ to assume a different value for the group. Maximum likelihood estimates are used for the parameters implicit in Y_1 and Y_2 and the test statistic then becomes

$$2\hat{I}(Y_1 : Y_2) = \sum_{ij} x_{ij} \log x_{ij} - \sum_i x_{i.} \log x_{i.} - \sum_j x_{.j} \log x_{.j} + N \log N \qquad (1)$$

where $i.i.$ (= 1,2) refer to rows and columns in Table III, respectively, x_i is a row total and $x_{.j}$ is a column total and N is a total number of observations in the table. $\hat{I}(Y_1 : Y_2)$ is also the estimate of the contingent uncertainty or information transmitted in Table III (see Garner, 1962). As N becomes large, $2\hat{I}(Y_1 : Y_2)$ is distributed asymptotically as χ^2 with 1 *d.f.*

It happens that $2\hat{I} = 28{\cdot}74$ and this is significant at the 0·005 level.

TABLE III

Frequency of H_I and H_{II}

Experimental group	Type of hypotheses H_I	H_{II}	Σ
First (complexity increase)	37	8	45
Second (complexity decrease)	12	33	45
Σ	49	41	90

The same statistical method was used to answer questions about the relationship of hypothesis type to success in solving. The relevant data are shown in Tables IV and V for the two groups. Each subject's score may appear in any one of four cells

TABLE IV

Frequency of H_I and H_{II} in the first group

Type of hypotheses	Unsolved G_3—G_6	Solved G_3—G_6	Σ
H_I	19	18	37
H_{II}	3	5	8
Σ	22	23	45

according to the type of hypothesis selected and whether the problem is solved. If the selection of H_I and the solving of the problem are independent events, then the table will be homogeneous (hypothesis Y_2); otherwise, the data are a sample from some unspecified multinominal distribution (hypothesis Y_1). Equation (1) gives $2\hat{I}(Y_1 : Y_2) = 0{\cdot}51$ with 1 *d.f.* for Table IV, which is not significant, and 3·87 with 1 *d.f.* for Table V, which is just significant on 0·05 level.

TABLE V

Frequency of H_I and H_{II} in the second group

Type of hypotheses	Unsolved G_3—G_6	Solved G_3—G_6	
H_I	9	3	12
H_{II}	14	19	33
Σ	23	22	45

While Tables IV and V involves data from actual games, Table VI involves data from the theoretical game. The theoretical game, G_7, was played only by those 45 subjects who were successful in actual games. The same statistical method was used.

TABLE VI

Frequency of H_I and H_{II} hypotheses in G_7 game

Type of hypotheses	Unsolved G_7	Solved G_7	Σ
H_I	11	10	21
H_{II}	0	24	24
Σ	11	34	45

Equation (1) gives $2\hat{I}\ Y_1 : Y_2 = 20{\cdot}99$ with 1 *d.f.* for Table VI, which is significant at the 0·005 level.

Summarizing the statistical results we can see that the relative probabilities of hypotheses of types H_I and H_{II} are different for the two experimental groups: in the first group (complexity increasing) the H_I type prevails while in the second group (complexity decreasing) H_{II} is the predominating type. For the actual games there is no statistically significant relationship between the type of hypothesis formed and the probability of succesful solution for the first group, while in the second group the dependence is just significant. However, the connection between the type of hypothesis employed and success is highly significant for the theoretical game: only H_{II} leads to the level of generalization required for more complex games.

Qualitative analysis of protocols

Hypotheses of the first type contain an element of chance associating them with trial and error methods, because they are not based on analysis of the problem situation. This points to their "mechanical" character. If the situation is obvious

in all actual variants, H_1 is viable. Yet subjects may play an optimal game without understanding the structure of the problem; in each new variant of the game he must look for a new hypothesis because the hypothesis verified in one variant turns false in another one.

If we come across a sequence of examples of H_1, we shall find that the relationship between individual hypotheses is very loose and purely external: it may be dependent either on time or an associations according to rules of the contrast, analogy, arithmetic series, etc.

The subjects formed the hypotheses H_1 according to their failure in a set. If they were unsuccessful and lost the set, they tried to arrive at another hypothesis not on the basis of analyzing the situation but intuitively only. This explains why, in general evaluation, no criteria were found by which hypotheses of the H_1 type could be arranged into a system.†

On the other hand in hypotheses of the second type the individual hypotheses only aim at the sub-goals and, appropriately, transform the original problem into another one. A sequence of hypotheses determines the whole action. The first sub-goal is reached quite empirically by the analysis of the last-but-one round. The situation here is viable as only small numbers of chips to be taken are left. Having solved this situation, the subject tries to find further sub-goals by recursion. When, for example, the subject has to solve the problem G_4 with 13 chips, he transforms it in the problem of how to solve the situation with nine chips, because he finds that if he arrives at the situation with four chips to be taken, he will always loose. But the problem with nine chips is transformed in to the sub-problem with five chips and, at last, he finds that the first move is decisive and that he must complete the chips taken to a multiple of four.

Within hypotheses of the second type some subjects are not able to arrive at the right sub-goal, or they cannot complete the chain between individual sub-goals and an actual draw. Or they might oscillate between two sub-goals, one of which

† *Examples of protocol*

Hypothesis 1: Game G_6; female M.M. Group II, Raven Matrices I.Q.: 121.

(a) I will try to take one chip more than you.
(b) Now I take a half less than you.
(c) Now I will take one chip less than you.
(d) Now I will always take one chip.
(e) I will take the same number as you.
(f) Now whenever you take the highest number of chips I shall take the smallest.

Hypothesis 2: Game G_6 was played: male R.H., exp. group II, Raven 127.

(a) I see, when seven are left I will lose. I must try to have my turn when there are six left.
(b) It is the number seven that is important. How to get to it? I must play in such a way so that the number is for you.
(c) Thirteen chips remain, it depends how many you take. Whatever you take the seven will remain to you.
(d) What will be the situation if I take four at the beginning? You can take one to five No. I will not take four, I will take one for you to get 13.
(e) I do not know yet if I can win every time. I understand it already but I must not make a mistake. Whatever you take at the beginning I must complete the number to such an amount that 13 will remain for you and then seven.

was false. Example: Game G_6: "I must reach the number 13" (wrong). "I must not reach the number 13" (right) "I must reach the number 13," etc. Sub-goals are of two kinds: to be reached by subjects (positive) or to be avoided by the subject (negative). Occasionally sub-goals were determined too narrowly or too broadly, i.e. too few numbers to be reached or to be avoided were determined and vice versa for too broad. In the example of G_3: "I must reach 6 to win." This is too narrow. Six or five would be connect. "I must not reach seven or eight to win," is too broad, because eight does not matter.

To play well a subject has to realise, at each draw, both which number of chips has to be avoided and which number should be achieved. Some of the less able subjects confused these two numbers. The successful subjects usually determinated the negative subgoal too broadly (e.g. to determine several numbers as decisive for winning although this did not matter).

Sub-goal solving of the problem by means of H_{II} hypotheses brings the subject to the understanding of the structure of the game. This is proved by the fact that almost all subjects who solved G_6 were also successful in G_7 (there were frequent comments such as: "now I could solve any game of that type").

Discussion

I shall try to explain why, in the first group, the H_I hypotheses prevailed while, in the second group, the H_{II} were used: the groups were equalized on the bases of results in maths and in Raven matrices. Success or failure in solving the actual games cannot be related to the type of hypotheses applied. In our opinion they depend rather on the stimulus situation and on structural relationships inherent in the problem. In such a simple game as G_3 a less ambitious approach is possible: in view of its simple rules, it is advantageous for the subject to try all the possible combinations of the set ("to take the same as you", "to take the opposite to you" and similar combinations) rather than to attempt the goal analysis. If the subject wanted to exploit the same possibilities while playing G_6, he would have to formulate a large number of H_I hypotheses which is obviously less effective than formulating the H_{II} type. As soon as the subject accepts this assumption and begins to form hypotheses of a certain type, his strategy becomes stable and he adheres to formation of a certain type of hypotheses even if it is no longer advantageous, i.e. he tries to solve the variant G_3 by means of sub-goal analysis because earlier he had sucessfully applied this analysis to G_6 strategy; on the other hand he solves more ambitious variants by means of H_I, if he made use of it for G_3 before.

Newell, Shaw and Simon (1959) have suggested a means-end analysis superficially similar to our sub-goal analysis based on H_{II} hypotheses. There is a certain analogy between their problem situation and that in our experiment. The problem of regular transformation of any expression A into any expression B, deducible form A, corresponds to our problem of how to transform (by playing) the initial position of G_n into the "winning position," in whatever way the investigator is going to move, and both are thus sub-goal analyses. In both cases there is a "diminishing of a difference": in our case the mth move narrows and finally nullifies the difference between the sub-problems before and after and after the mth move.

However, there are differences as well. The necessity of recursion to determine the whole action in case of H_{II} is not analogous with Newell. The main difference is that the Newell general problem does not enable us (with the given list of operators unlike the Hao-Wang system) to find the algorithm which would solve the problem. Our general problem on the other hand, is strictly solvable: this was revealed in its different presentations: the subject in our experiment succeeded in solving the individual assignment only after having solved the whole general problem, i.e. not only after having won an individual set. In the Newell experiments, the subject succeded in solving the assignment when one of the instances had been solved, i.e. the given expression was transformed into another given expression. This means that the subject was not requested to reach the general principle of regular transformation: this general "principle" was installed,with a certain limitation, into the machine simulating the behaviour of the Newell problem solver and it did not function, and even could not function, in all cases. These differences restrict the analogy between the results from the Newell experiments and the results of our experiment, and our results may not be found in other classes of problem.

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RECALL OF DIGITS PROJECTED TO TEMPORAL AND NASAL HEMI-RETINAS

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Fourteen right-handed, right eye dominant subjects recalled digits when different ones were projected simultaneously to either temporal or nasal retinas. The principal findings were: (a) Recall of digits projected to nasal retinas was significantly better than when projected to temporal retinas; (b) information projected to the right eye was recalled significantly better than that projected to the left eye. It is shown that the relative ineffectiveness of the recall of input from the *non-dominant eye* can be attributed almost wholly to the relative inefficient recall of digits projected to the *left temporal* retina.

Introduction

Sampson and Horrocks (1967) have shown that if there is competition between inputs from each eye when the subject is required to recall digits presented visually, it is resolved in favour of the dominant eye. In their experiments, different digits were projected simultaneously to each eye, and subjects recalled those projected to the dominant eye more accurately than those projected to the non-dominant eye.

A problem arising from this observation of theoretical interest, is to explain the relative ineffectiveness of the *non-dominant eye*. In view of the well-known partitioning of the retina into nasal and temporal regions, each with different central projections, it is possible that the result obtained because information projected to either one of these regions or perhaps both, is recalled less effectively than when it is projected to corresponding regions of the dominant eye. To examine this possibility, different digits were projected simultaenously to either temporal or nasal hemi-retinas in the present experiment, and the consequences of the resulting binocular rivalry for short-term memory assessed.

Method

Subjects

These were 14 male university students. Their ages ranged from 19 to 28 years (mean = 22 years) and they were all right-handed and right eye dominant. Handedness was determined by an individually administered questionnaire composed of 20 items suggested by Humphrey (1951) as sensitive indices of manual preference, and three additional items designed by Gillies, MacSweeney and Zangwill (1960). Sighting preference was determined by the finger aiming test (Buxton and Crosland, 1937) and use of a card with a 4 cm. square hole in it. An AOA chart was used to check visual acuity, and each subject had normal and equal acuity in each eye.

Apparatus

A magazine-load automatic projector was connected to two interval timers permitting control of stimulus duration and interval between stimuli. Slides were "gravity-fed" into the projector from a vertically placed slide holder. The timers controlled the shutter mechanisms of the projector. A day-view screen upon which stimuli for each eye were projected was placed 1 m. from the subject's eyes. The visual stimuli were arabic digits from 1 to 9, and they were projected in pairs either side of a fixation point so that in terms of visual angle, the centre of each digit was 5° from the fixation point, and a circle of 6° took in each pair of digits entirely. All stimuli were mounted in pairs on 5·1 cm. × 5·1 cm. slides. The fixation point was permanently mounted on the day-view screen. The subject's head was held in a head-rest, and he wore a trial frame when viewing the stimulus pairs. The trial frame's eye pieces were fitted with half occluders which meant that after the subject was seated and had fixated the dot on the screen the occulder could be rotated to either nasal or temporal position and adjusted so that the subject could see the fixation point and only one digit with each eye. Two viewing conditions were used:

Condition I: the occluder for each eye in the temporal position so that the stimulus is projected onto the *temporal* retina.

Condition II: the occluder for each eye in the nasal position so that the stimulus is projected onto the *nasal* retina.

Procedure

The 14 subjects were divided into two groups. Odd numbered subjects viewed stimuli temporally and then nasally. Even numbered subjects viewed stimuli nasally, then temporally. Each subject was given 20 trials under each viewing condition. A trial consisted of four pairs of digits followed by a red flash on the screen signalling the end of a trial. The combination of digit pairs on a trial was identical for each subject and on no trial did the same digit appear twice. A random order for individual trials was determined in advance for each subject.

The subject was instructed to call out all digits he could remember in any order, as soon as possible after the signal to respond. This signal was delivered 0·8 sec. after the last stimulus pair on a trial. The interval between the end of each pair of stimuli and the onset of the next pair was also 0·8 sec., while the duration of each stimulus pair and the red flash was 0·3 sec.

Results

Table I summarizes results of analysis of variance of percentage of digits recalled correctly. These percentages were transformed, angle = arcsin $\sqrt{\text{percentage correct}}$ (Snedecor, 1956) before analysis. The principal findings were as follows:

(a) Order of presentation of the viewing conditions, i.e. whether digits were projected to nsaal or temporal retinas first, had no significant effect on recall.

(b) There was a significant improvement ($P < 0{\cdot}001$) in recall under the second viewing condition encountered in the experimental session.

(c) Recall of digits projected to the nasal retinas was significantly better ($P < 0{\cdot}01$) than when they were projected to the temporal retinas. Order of efficiency of recall was right nasal (89·4 per cent), left nasal (88·0 per cent), right temporal (87·8 per cent), left temporal (82·4 per cent) retinas.

(d) Information projected to the right eye was recalled significantly better ($P < 0{\cdot}01$) than that projected to the left eye.

TABLE I

Analysis of variance of percentage of digits recalled correctly

Source of variation	MS	*d.f.*	*F*
Groups (A)	4·80	1	—
Subjects within groups	182·44	12	
Total between subjects		13	
Nasal vs. temporal (B)	119·48	1	13·813†
Order of conditions (C)	587·60	1	67·931‡
Left eye vs. right eye (D)	102·60	1	11·861†
A × D	5·41	1	—
B × D	15·23	1	1·761 n.s.
C × D	14·81	1	1·712 n.s.
D × subjects within groups	21·74	12	2·513 n.s.
Residual (error)	8·65	24	
Total within subjects		42	
Total		55	

† Significant beyond the 0·01 level of confidence.
‡ Significant beyond the 0·001 level of confidence.

Discussion

Many short-term memory experiments (e.g. Brown, 1958; Mackworth, 1962, 1963, 1964; Sperling, 1961, 1962) have shown that information to be recalled is available to the subjects for only a limited time. This being so, the more quickly the subject responds, the more he recalls, which suggests that in the present experiment these results were obtained because digits projected to nasal hemi-retinas were processed more quickly (and therefore recalled better) than those projected to temporal hemi-retinas.

Bower and Haley (1964) using a very different technique, arrive at a similar conclusion. In their experiment, they showed that when stimulus durations are very brief in conditions involving binocular rivalry, the rivalry is resolved in favour of nasal hemi-retinas. As stimulus duration is increased to the order of that used in the present experiment, rivalry is resolved in favour of the dominant eye. However, the present findings make it clear that the superiority of the dominant eye is almost wholly attributable to the relatively inefficient recall of digits projected to the left temporal retina.

It is difficult to account for this solely within the context of the experiment. Subjects seem to have taken longer to process information from this region than from any of the other retinal areas. One possible reason for this may be that suggested by Mishkin (1962) to the effect that all visual information from the left hemisphere must be transferred to the right temporal cortex before it is processed. Assuming also that in the optic system, crossed fibres are more effective than uncrossed (Bower, 1966; Bower and Haley, 1964; Crovitz and Lipscombe, 1963;

Sampson, 1962) an advantage would thus be given to input directly to the right hemisphere, and hence to the dominant eye.

This experiment extends the range of conditions in which eye dominance plays a role in recall. Until now, eye differences in recall have been demonstrated only when the stimuli were *perceived* as overlapped when projected binocularly (Sampson and Spong, 1961*a*, *b*; Sampson and Horrocks, 1967). It now appears that the effect may be demonstrable in any perceptual situation which is novel and or difficult.

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DIRECT MEASURES OF SHORT-TERM VISUAL STORAGE

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Two experiments, involving seven conditions, explored the use of direct measures of visual persistence. In each, the subject was asked to judge if an intermittent stimulus appeared perceptually continuous, or whether it completely faded before the next presentation occurred. The first experiment showed that visual persistence was set at approximately 250 msec. for a recycling presentation of a circle in a tachistoscope; in another task employing a moving opaque slit passing back and forth over a circle, persistence times averaged 50 msec. longer. Reducing luminance by 2 log units increased persistence only slightly, though removing the adapting field increased it by over 100 msec. The second experiment, using the repeating circle, varied the duration of the stimulus, and compared monoptic with dichoptic presentations. Visual persistence was found to be independent of stimulus duration over a range of 4 to 200 msec., where all durations were above recognition threshold for the stimulus. Persistence was unaffected whether the stimulus was repeatedly presented in the same eye or alternated between eyes, strongly suggesting that the storage is central. Finally, a re-analysis of Dodwell and Engel's paper on stereopsis suggests that their effects can be adequately explained by visual persistence of the asynchronous stereo pairs, rather than a more complex fusion model. All of these results strongly support the use of visual persistence as a direct measure of short-term visual storage.

Introduction

Although the "persistence of vision" has been recognized for at least a hundred years (Helmholtz, 1866, reprinted in 1962), interest in the topic has grown since the publication of Sperling's influential monograph (Sperling, 1960). Visual persistence refers to the possibility that the neural representation of a stimulus persists for some time after the offset of the physical energy impinging on the receptor surface. Using the method of partial report, Sperling found that a brief visual stimulus (e.g. 50 msec.) persists perceptually for 300 msec. or more (and considerably longer if a dark post-stimulus field is employed). Keele and Chase (1967), also using partial report, estimate this storage time as 250 msec., while Averbach and Coriell (1961), using both partial report and a metacontrast technique, obtain values of approximately 250 msec. in both cases. Other studies of short-term storage have been made by Mackworth (1963), Eriksen and Collins (1967), and Smith and Carey (1966) with broadly similar results.

The partial report technique is an indirect indicator of persistence. The assumption is made that under total report instructions, the subject is limited in the amount of information he can recall from the stimulus because of processing and memory

limitations, and not because he failed to perceive most or all of the content of the stimulus. To eliminate this source of variance, Sperling asked his subjects to report only part of the content, that part designated for report at varying times after the stimulus was presented. He found, as have others, that the indicator could be delayed several hundred msec. and still permit greater accuracy than under full report conditions. If the subject is asked to report all of 12 items, and is correct on only four of them, he has only 25 per cent of the information available at the time of report. If he is asked for only one row of four items, and the subject reported all four, then at the time the indicator arrived, the subject must have had 100 per cent of the content of the stimulus available.

This procedure, and all of its variations, is indirect in that persistence is inferred from the decay in accuracy of report of partial information, under the sampling assumption spelled out above. It is also a very time-consuming experimental procedure requiring many hours of the subjects' time. A more direct procedure is available, though it has not been seriously exploited. The most direct version would be to present the subject with a brief flash from an optical source of very precise rise and decay times, and ask the subject to rate or estimate its apparent duration. If brief stimuli actually persist for several hundred msec. beyond their offsets, such persistence should be measurable in estimates of apparent duration. This procedure is a very unreliable one, however, since the subject has no good standards for determining the absolute values of very brief pulses.

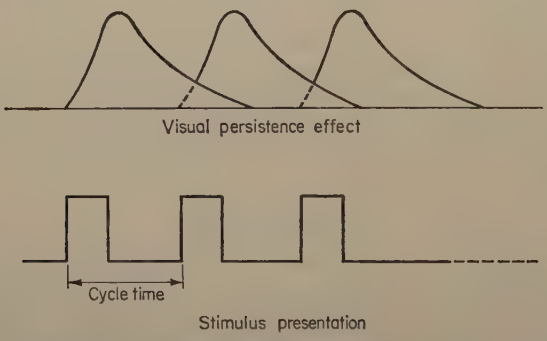

FIGURE 1. A hypothetical view of visual persistence as a result of a repeating stimulus. The subject's response of continuity or discontinuity is based upon whether he judges the persistence reached zero from the first flash before the second flash occurred.

Sperling (1967) reports a variation of it, although he has not used it to determine persistence. At the same time as the subject is given a brief visual flash, he hears two auditory clicks, and is told to adjust the interval between the clicks to match the apparent duration of the visual pulse. Under conditions in which little or no visual persistence is to be expected (visual noise follows the offset of the stimulus immediately) the click interval is set at just slightly longer than the actual stimulus duration. Unfortunately, Sperling does not report data for conditions without visual noise in which visual persistence would be expected.

A second technique, several variations of which are used in the experiments to be described here, presents the stimulus repeatedly with specific delays between each

repetition. The subject is asked to judge whether the perception of the stimulus had completely faded before the next flash occurred. The time interval can then be varied in order to locate the threshold of persistence.

Two experiments are reported; in the first a comparison is made between two tasks designed to provide direct measures of storage times, while the second investigates three paradigms to measure storage times under monoptic and dichoptic conditions. The intention of the second experiment is to relate the concept of storage to the findings of Dodwell and Engel (1963) who have demonstrated the production of stereopsis from pairs of monocular stimuli that are presented alternately, and do not overlap in time. It is hoped that these experiments will demonstrate the feasibility of direct measures of storage times, and demonstrate how these measures may be applied to a problem in form perception.

Experiment I

The two tasks employed in this study are both intended to provide a measure of storage time, but are otherwise quite different, since Task 1 uses a moving-slit apparatus, while Task 2 employs repetitive stimuli in a tachistoscope. Task 1 has been described previously by Haber and Nathanson (1968), in more general terms, and is based on one used by Parks (1965). Briefly a narrow vertical slit oscillates, in a horizontal plane, immediately in front of a stationary black-on-white form. The subject views the apparatus with steady fixation. When the rate of oscillation is sufficiently high, a whole form is perceived even though the slit exposes only a small portion of the form at any one instant. As the frequency of oscillation is increased from zero, Haber and Nathanson (1968) have reliably observed the following stages: the shape of the form may be inferred, but the form is not *perceived* in the true sense of the word; the form is perceived as a whole, but it does not persist between successive sweeps of the slit, so that it is seen as present intermittently; the form is seen as both whole and as continuous in time. We utilize the beginning of the third stage as indicating the point at which short-term storage time is exactly equalled by the sweep time of the slit.

Task 2 uses a brief trachistoscopic presentation of the whole test form, followed by a long exposure of a blank adapting field at the same luminance as the test field background. This cycle is repeated continuously, and the frequency is varied to find the frequency at which the form appears to be present continuously, rather than appearing intermittently. Again, the cycle time at this point provides a measure of how long the short-term storage must last for this phenomenally continuous percept to occur. As in the first task, we assume that the subject is capable of making meaningful "psychological reports" (Natsoulas, 1967) concerning his percepts. Figure 1 presents a hypothetical view of the subjects' report in Task 2.

Method

Subjects. Twenty-four experimentally naïve subjects were employed. Nine were graduate students in psychology at the University of Rochester; most of the remainder were non-psychology undergraduates. All possessed normal or corrected vision. Two potential subjects were rejected due to inability to obtain the "illusion" which is the basis of Task 1.

Apparatus and experimental tasks. The apparatus for Task 1 has been described by Haber and Nathanson (1968). It consists of a vertical oscillating slit driven by an electric motor, the rate of oscillation being precisely controlled by the experimenter by means of a continuously variable speed control on the motor. The slit, cut in a large piece of white cardboard, was 5 cm. high and 0·2 cm. wide; it oscillated horizontally through a sweep of 4 cm. The stationary test stimulus was located 0·1 cm. behind the slit, at the midpoint of the sweep, and comprised a circle, 3 cm. in diameter, drawn in black with a 0·1 cm. line, upon a white card. The contrast ratio was approximately 90 per cent. Due to its size, only a small portion of the circle was exposed at each instant in time as the slit swept back and forth across the circle; the mean proportion of the time for which each element of the circle was exposed to the subject was 1 in 20. The apparatus was viewed from a distance of 30 in. and was illuminated from directly in front. Fixation upon the centre of the circle may be maintained quite easily under these conditions.

The apparatus used in Task 2 was a Scientific Prototype two-channel tachistoscope (model 800F). A circle, identical to that used in Task 1, was exposed in the test channel, followed by an exposure of a blank adapting field, at the same luminance, in the second channel; this sequence was repeated continuously. The ratio of the exposure durations for the stimulus and the blank channel was always maintained at 1 to 20, while the length of the cycle (test plus blank field duration) was varied to find the threshold for apparent visual persistence for the form. The length of cycle time at this point provides a measure of the short-term storage time required to produce this phenomenal effect. As in Task 1, the viewing distance was 30 in. Durations of the tachistoscope were calibrated using a photomultiplier tube and oscilloscope. Calibration is necessary for extreme duty cycles (on to off ratio of more than 1 to 10) with this tach.

Procedure. Two versions of each task were used: high and low luminance (0·5 and 50 ml., respectively). Each subject was tested with each of the four resultant task-luminance combinations; the subjects were arranged in six 4×4 Latin squares which were formed using all 24 possible orders of the four experimental treatments. All four treatments were given in a single experimental session of about 2 hr.; 10 threshold measurements were taken in each, using alternate ascending and descending series in the method of limits. Steps of 20 msec. in the cycle time were used.

Measurements of storage time were also made in Task 2 with 2 of the subjects after the main experiment was finished under conditions where a totally dark adapting-field alternated with the test stimulus (at 50 ml.); all other conditions were as before.

For Task 1, the subject was instructed, "While the experimenter adjusts the speed of the moving slit, maintain fication steadily at the mid-point of its movement. You may be able to see a circle behind the slit; on each trial, which will be indicated by the experimenter saying 'now,' say 'C' if the circle appears to be present and continuous in time, 'D' if it appears to be present but discontinuous in time, or 'zero' if you do not see the circle. The perceived continuity of the circle need not be perfect: it may appear to fluctuate somewhat without losing the appearance of being present continuously. However, you should rate the circle as discontinuous if you can perceive a blank interval, however brief, between presentations of the circle. Do not allow the presence or absence of apparent movement of the circle, or of flicker in the background, to influence your report of what you perceive, as these effects are irrelevant to the phenomenon under study."

For Task 2, the subject was instructed, "Fixate the circle, which will be presented in the tachistoscope, and say 'C' if the circle appears to be present continuously, 'D' if it appears to be discontinuous, or 'zero' if you do not see the circle." The remaining instructions were identical to those for Task 1.

The subjects were encouraged to ask questions if they did not fully understand the task. While they were kept unaware of specific experimental hypotheses, they were always informed that even if the test stimuli sometimes appeared continuous in time, they were not physically so. Thus, while subjects had a specific set to look for continuity or visual persistence, they were aware that perceived continuity was always illusory. It was stressed

that there were no "right" or "wrong" judgements, and that a direct report of the subject's perceptual experience was desired.

Results

The ratings of "C" and "D" given in each task for the ascending and descending presentations were used to calculate each subject's persistence threshold for storage time under each treatment. No "zero" responses occurred. Reversals of ratings occurred in a few series (about 15 per cent), and were usually confined to two or three trials. The ten threshold values were averaged for each subject-treatment combination, to yield four mean storage times for each subject (see Table I).

TABLE I

Mean storage times for Task 1 (moving-slit apparatus) and Task 2 (tachistoscope), under high and low luminance conditions, and for two subjects in Task 2 in which the adapting field was off

Task	1		2		
Luminance level (ml.)	50	0·5	50	0·5	50 (dark interval)
Mean storage time (msec.)	319·1	338·9	261·2	295·7	405·0
Standard deviation (between subjects) (msec.)	33·5	31·6	25·7	29·3	37

The ten thresholds on which each mean is based were found to have a standard deviation of 31·2 msec., within subject-treatment combinations. To facilitate comparison between Task 1 and Task 2, storage times in each task were taken as the total time at persistence threshold, i.e. the sum of stimulus and blank field exposure durations, rather than blank field duration alone. (If storage time were expressed as blank field duration alone, this would reduce the calculated storage times by only 5 per cent.)

The mean storage times obtained above were submitted to an analysis of variance. An identical analysis was also performed on the standard deviations of these storage times. The first analysis showed that the storage times obtained in Task 1 were significantly longer than those obtained in Task 2 ($F = 11{\cdot}5$, 1 and 15 *d.f.*, $P < 0{\cdot}01$). For both tasks, storage times were longer under low, as compared to high, liminance, but this difference failed to reach statistical significance ($F = 3{\cdot}14$, 1 and 15 *d.f.*, $P > 0{\cdot}05$). However, a sign test of this difference indicated that it was significant at the 0·05 level (32 positive differences in 48 comparisons, $P = 0{\cdot}03$). The interaction between luminance level and tasks was not significant ($F = 2{\cdot}1$, 1 and 15 *d.f.*, $P > 0{\cdot}05$).

The first analysis also showed that subjects differed significantly in their mean storage times ($F = 8{\cdot}5$, 23 and 15 *d.f.*, $P < 0{\cdot}01$). While this factor is confounded with order of testing, the latter is shown to be irrelevant, since no differences were found over the successive stages of the four experimental treatments for each subject ($F < 1$), and individual differences were equally pronounced on the first task performed by each subject. Pooling each subject's storage times for high and

low luminance levels, the correlation (rho) between storage times on the two tasks was found to be 0·37, a value which is significantly greater than zero ($P < 0{\cdot}05$).

The second analysis, based on the standard deviation of the ten threshold estimates for each subject-treatment combination, showed that variability of performance was not significantly affected by any factor except subjects ($F = 6{\cdot}6$, 23 and 15 *d.f.*, $P < 0{\cdot}01$).

The mean storage time for dark adapting-field conditions (see last column on Table I) was found to be 390 and 420 msec., for two subjects, with a combined standard deviation of 37 msec. (The mean storage times for these subjects were 273 and 240 msec. when the test and adapting fields were both at 50 ml.)

Discussion

The results show that in four experimental situations subjects are able to make judgements of apparent visual persistence, with a standard deviation of about 10 per cent of their mean values. Thus, direct measures of storage time are at least capable of providing reliable data, despite the use of inexperienced subjects. No difficulties were found with this technique, it yields values which are consistent with other approaches to the study of short-term storage, and its simplicity, speed and reliability suggest it is a useful method for examining the factors which determine the efficacy of visual storage mechanisms. For example, we have readily found increased storage times to occur when a dark interval is substituted for the lighted adapting field in Task 2, as would be expected from Sperling's (1960) results.

Regarding the validity of the data, the results indicate that on average a form presented for approximately 15 msec. persists perceptually for about 300 msec. This agrees well with the estimates of Sperling (1960) and of Averbach and Corriell (1961) for similar, lighted adapting field conditions: 300 and 250 msec., respectively. While test stimulus durations are longer in these studies than in the present case, Sperling (1960) has suggested that for above-threshold stimuli, storage time is almost independent of stimulus duration, so that these studies may meaningfully be compared with the present findings. (We also have found—see Experiment II—the storage time to be largely invariant under wide changes in stimulus duration.) Thus we believe that the two tasks employed in the present study do in fact measure the visual persistence of brief stimuli in much the same way as do the more elaborate but cumbersome partial report and metacontrast methods.

The storage effect is very robust under changes in most stimulus parameters (except for adapting-field luminance, according to the results of the dark-condition data on two subjects), and this is illustrated by the present finding that a reduction in test stimulus luminance of 2·0 log units hardly alters the storage time. The tendency for storage time to increase by 20 to 30 msec. for the low-luminance condition (which is about 0·5 to 0·7 log unit above threshold for recognition of the circle), if this is a genuine effect, may result from the fact that the adapting field is also of lower luminance and thus less likely to obliterate the image of the circle. A lower level of light adaptation is also known to reduce the temporal sensitivity of the eye appreciably, by increasing the critical duration for visual integration of luminance (Barlow, 1958), and by lengthening the perceptual latency of the visual system in some cases (Standing, Dodwell and Lang, 1968). These effects would

presumably reduce the ability of the eye to detect the discontinuity of the intermittent test stimulus in the present study.

A more surprising feature of the results is that storage time as measured by Task 1 was approximately 50 msec. longer than that measured by Task 2, although both estimates are plausible in view of other studies of short-term storage. While the optical quality of the image in Task 1 is inevitably lower than that in Task 2, and the subject must refrain from making eye movements if the image is not to vanish altogether, the moving-slit apparatus does eliminate the slight but detectable transient in field luminance which invariably occurs when switching fields in a tachistoscope. It may be that this transient acts to a slight extent as a "backward masking" stimulus (Raab, 1963) to reduce the adequacy of the image stored in the visual system. Various other possibilities exist: subjects may adopt a lower criterion for visual persistence in the first task, or the visual system may respond more slowly to a form that is presented a small section at a time, rather than all at once (with repetition rate held constant). This would be analogous to the finding in flicker-fusion studies that flicker is harder to perceive when a dim light, or one that is spread out over time, is employed (Bartley, 1963).

While it would be valuable to determine the reason for the longer storage times in Task 1, the finding of fairly similar values for Task 2 goes some way to confirm the analysis made by Haber and Nathanson (1968) of Parks' (1965) results. The "camel seen through the eye of a needle" effect is not based on post-retinal reintegration of visual forms, as Parks suggested, but on the "painting" of these forms out across the retina, and their slow decay in the short-term store, so that the next presentation of the stimulus occurs when there is still an appreciable trace in the visual system from the previous presentation. However, although the moving-slit task is easier for subjects to judge, according to their reports, Task 2 is preferable as a technique since it provides much greater flexibility in varying parameters.

The differences found between subjects in storage time are no larger than individual differences typically found in psychophysical studies. Sperling (1960) has shown that the stimulus trace decays in a gradual, monotonic curve; therefore subject in a very real sense must adopt a criterion, to be able to specify the interstimulus interval at which the trace just decays to zero before the next presentation of the stimulus. The reliability of judgements in this study (and post-experimental reports by the subjects) suggest that they are able to impose this cut-off on their perceptual experience without excessive difficulty.

The low (but significant) correlation of storage times between the two tasks suggests that the tasks were sufficiently different so that subjects reacted to them in somewhat different ways. Until more attention is paid to individual differences in psychophysical tasks, further interpretation of the magnitude of this correlation is highly speculative.

Experiment II

One aim of this experiment is to study visual storage time as influenced by duration of the test stimulus, which Sperling (1960) suggests should not appreciably alter the effect. Another aim is to relate the results from three versions of

tachistoscopic task employed in Experiment 1 to measures of "stereopsis time" obtained in studies where the two eyes are stimulated, asynchronously, with the two members of a stereo pair (Dodwell and Engel, 1963; Efron, 1957).

Three paradigms were employed in this study; they are illustrated in Figure 2, together with the paradigm used by Dodwell and Engel (1963) to investigate binocular fusion. In the latter case, the dependent variable was the appearance of stereo depth in a random brightness field (Julesz, 1965), such that the depth was perceptually continuous, whereas the dependent variable in the present paradigms is the appearance of perceived continuity of non-stereo stimuli. In both cases, it is convenient to use Dodwell and Engel's notation of SI and LI, respectively, to refer to the "short interval" within pulse pairs (or the duration of a single pulse, as in paradigm 1), and the "long interval" between each pair of pulses (or, the inter-pulse interval, as in paradigm 1).

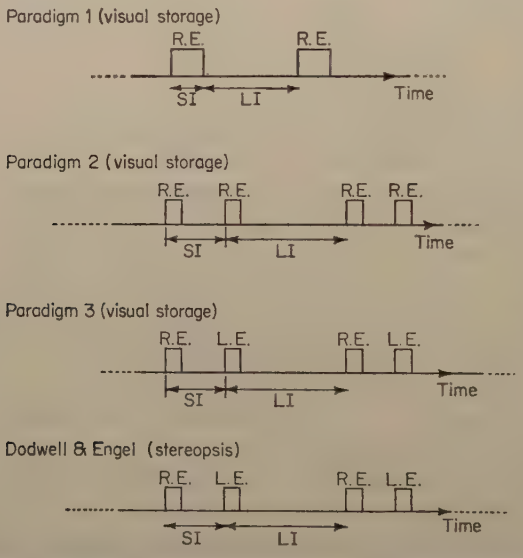

FIGURE 2. Presentation paradigms used in Experiment 2. The abscissa represents the presentation of the test stimulus to the right eye (R.E.) and left eye (L.E.), respectively. An adapting field (not indicated) is present between each pulse of the test stimulus.

Method

Subjects. Three new subjects were used, one each in paradigms 1, 2 and 3. All were experimentally naïve and possessed normal uncorrected vision.

Apparatus and tasks. A three-channel Scientific Prototype tachistoscope (model GA) was used. In paradigm 1, single repeated exposures of the test stimulus (a circle, identical to that used in Experiment 1), are presented monocularly in one channel of the tachistoscope for SI msec., followed by a blank adapting field for LI msec. in the second channel. In paradigm 2, the stimulus is presented monocularly in the first channel, as in paradigm 1, except that two 10 msec. pusles separated by SI msec. are used. In paradigm 3, two 10 msec. pulses separated by SI msec. are presented to the right and left eye, respectively, using two channels of the tachistoscope and an arrangement of Polaroid filters. In paradigms 2

and 3, the interval, LI msec., between pairs of pulses is filled with an adapting field. Test and adapting field luminances were set at 20 and 10 ml., respectively, for all paradigms.

Procedure. Using the pardigms described above, the procedure followed that of Task 2, Experiment 1. The threshold for perceived continuity of the test stimulus, at each of 12 SI durations ranging from 4 to 200 msec., was determined by varying LI in steps of 20 msec., and obtaining the subject's judgement of continuity versus discontinuity at each LI value. Threshold is taken as the LI value which produces 50 per cent "continuous" responses. Two ascending and two descending series were made of each threshold, over a range of 8 LI steps.

Results

The data for the three paradigms are displayed in Figure 3, in which changes in persistence (LI) threshold are plotted against log exposure duration (SI). It is

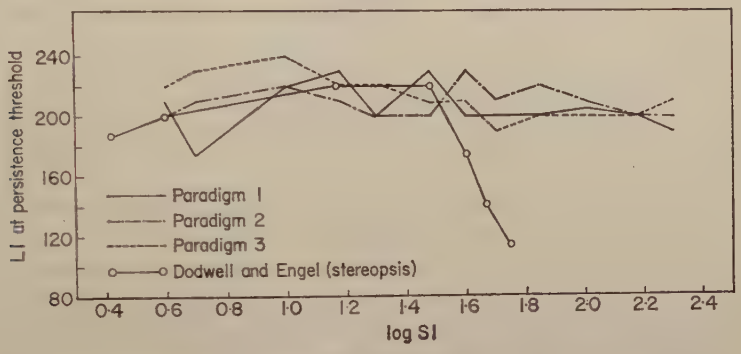

FIGURE 3. Persistence threshold (maximal LI for which subject judges continuity of persistence) as a function of log SI, for each of the three paradigms of Experiment II. Each curve is for one subject, with each point based upon four threshold determinations. Also: threshold values of LI for stereopsis, redrawn from data of Dodwell and Engel.

clear that for each paradigm, persistence is invariant over a wide range of stimuli durations. Further, each of the three procedures yields approximately the same storage time (220 msec.), even with different methods of presenting stimuli.

Discussion

Experiment 2 yields several important results. That visual persistence is independent of exposure duration supports a basic argument of Sperling (1960, 1967) regarding the independence of the duration of a short-term visual storage from the duration of the stimulus that created it. Sperling's evidence rested primarily on indirect measurement of storage—the probability of reporting part of the stimulus array correctly as a function of delay of the indicator, varied over a range of 10 to 500 msec. Recently, Raymond and Glanzer (1967) reported an apparent failure to replicate this invariance under conditions of presence and absence of either a pre- or a post-visual noise field. However, they use very low luminance levels (1·5 log unit lower than Sperling's), so it is quite likely that the stimulus array was at or below threshold for the briefest flashes. There is no reason to believe that persistence of storage should be as long for a subthreshold flash as for a supra

threshold presentation, and certainly, there is nothing in Sperling's work to suggest this prediction.

Paradigm 3 provides a test of a central vs. peripheral locus of visual storage, and the results strongly support a central interpretation. Using monocular presentations (paradigm 2), the stimulus must be repeated in the same eye within 220 msec. for the subject to judge it as continuous, and any delay beyond 220 msec. elicits a response of discontinuous. However, using dichoptic presentations, a delay as long as 440 msec. for either eye will still produce continuity, as long as the other eye also received a flash halfway through that time interval. Hence, it is not the individual receptor that must be stimulated at least as often as every 220 msec., but some central locus that is combining inputs from the two receptors. This finding does not eliminate the psosibility that the neural persistence may be retinal, in the sense that a brief (but above threshold stimulus flash) may continue to produce excitation for several hundred msec. which is transmitted to the brain. However, the subject's judgement of persistence (and in Sperling's work, his processing of information) must be based upon some central events, and not simply upon persistence of retinal stimulation. This argument, among other things, makes the after-image an improbable mechanism for visual persistence underlying short-term visual storage.

Figure 3 shows that the short-term visual storage effect lasts at least as long as the intervals over which Dodwell and Engel (1963) were able to obtain stereopsis. It seems that their results, while empirically correct, reflect storage processes and not mechanisms of binouclar fusion as these authors believe. The assumption by Dodwell and Engel that they have demonstrated the quantitative features of a central mechanism for stereopsis appear unwarranted. Our view is that, whatever the mechanism for perception of depth as a result of disparity between two dichoptic stimuli, all that is required is that the two inputs be perceptually present at the same time, a point which was clearly recognized by Munsterberg (1896). The present study suggests that this condition may easily be fulfilled, due to the storage effect, at interstimulus intervals where Dodwell and Engel have found stereopsis to occur. We also note that Kahneman, Norman and Kubovy (1967) have found perfect binocular summation in an acuity task when two successive flashes of 160 msec. each are presented dichoptically. Again, sizeable dichoptic temporal integration must be occurring even though no special mechanism for stereopsis can be involved.

Julesz (1965) has produced evidence, from a masking study, that the time constants for the perception of depth are longer than for the perception of form in the individual stereo stimuli. This finding is to be expected: the temporal sensitivity of a higher-order process (stereopsis) can only be expected to fall below that of the constituent processes (form perception). It also tends to support our contention that the threshold curve of Dodwell and Engel indicates the extent of visual storage (under the rather different conditions of their study), rather than the limits of the central nervous system's ability to integrate asynchronous stereo information to infer depth, since it suggests that failures in perceived continuity will occur first in the perception of the monoptic forms, as LI is increased, while the sensation of depth can persist at higher values of LI. Thus, we infer, the threshold value of LI will not reflect the temporal properties of the mechanism for stereopsis.

Conclusions

The two experiments taken together support the following statements and interpretations.

(1) Visual persistence can be directly measured by asking subjects to judge when a repeating stimulus has completely faded before its next appearance. These judgements are stable within and between subjects.

(2) This stability of persistence was assessed in two somewhat dissimilar tasks with remarkably similar overall results.

(3) The thresholds for persistence were on the same order of magnitude of those for the indirect assessment of the duration of short-term visual storage (250 to 350 msec.) so that it seems likely that measurement of visual persistence is a direct assessment of short-term visual storage.

(4) Visual persistence is slightly longer (20 to 30 msec.) when luminance is reduced by 2 log units. It is markedly longer (120 to 180 msec.) when no adapting and background field is used. Both of these findings agree both with data on short-term visual storage, and the general effect of luminance changes in the eye.

(5) Visual persistence is independent of the exposure duration of the stimulus over a range of 4 to 200 msec. This supports Sperling's findings for above threshold stimuli.

(6) Visual persistence is the same whether the repeating stimulation comes in the same eye, or alternates between eyes. This strongly supports a central locus for the effects of persistence and of short-term visual storage.

(7) Finally, Dodwell and Engel's work on binocular fusion and the perception of depth can be much more parsimoniously explained as the persistence and decay of dichoptically presented stimuli, rather than through some special properties of stereopsis.

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STIMULUS CHANGE EFFECTS OF FORCING ON MAZE ARM SELECTION IN HAMSTERS

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Hamsters were forced to enter one arm of a T-maze on four succesive runs by means of either a grey door of the same brightness as the rest of the apparatus or by a black or white door. During four successive free choices the subjects entered the previously blocked arm significantly more often following forcing with the black or white door.

Introduction

Dember and Fowler (1959) have shown that "forcing" a rat to enter one arm of a T-maze by blocking the alternative will tend to increase alternation on a following "free" trial without any forcing barrier present. Dember (1961) suggested that this effect could be accounted for by increased attractiveness of the previously blocked alternate arm due to the stimulus change produced by intertrial removal of the forcing barrier. Tentative evidence for this view was produced by Hughes (1966) who, using rats, found more initial orientation towards and partial entries of the alternate arm of a T-maze following forcing with a wooden barrier than following either no forcing or forcing with a "less perceptible" transparent barrier. (It was argued that intertrial removal of the transparent barrier represented the lowest degree of change.) The present study was designed to retest Dember's explanation. It was felt that removal of a black or white barrier from a mid-grey T-maze would represent a higher degree of change than removal of a mid-grey barrier from the same apparatus. As Dember and Millbrook (1956) have shown that arm preferences are directly related to the amount of intertrial brightness change, it was hypothesized that there would be a greater tendency to choose the arm previously blocked with a black or white forcing barrier in the course of four successive free trials.

Method

Subjects and apparatus

The subjects were 12 Golden Hamsters (8 male, 4 female) about 7 months old, kept in individual cages in the experimental room. The apparatus was a wooden T-maze with walls 6·5 in. high and a startbox 10 in. long. The stem was 24 in. long and the two arms

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were each 18 in. long. At the end of each arm there was a 10-in. long goal box of the same width and height as the arm with which it was connected by a 4 × 4 in. opening. The width of the maze, startbox and goalboxes was 6 in. throughout. The whole apparatus was covered by hinged, wire-gauze lids and its interior was painted mid-grey. The arms could be blocked off from the stem by painted, aluminium guillotine doors.

Procedure

The general procedure for each subject involved a series of four successive forced trials followed by four successive free choices for eight consecutive days. There were two forcing conditions. In condition G the subject was forced left or right four times by means of a barrier of the same brightness as the rest of the maze, i.e. mid-grey. In condition B/W, it was forced four times with a black (for half the series) or a white barrier (for the other half). In both conditions each forced run was followed by 30 sec. confinement to the entered arm, and then by 30 sec. back in the startbox. At the end of each series of forced trials, the subjects were given four successive free choices being confined to the chosen arm and then the startbox for 30 sec. at the end of each choice. All subjects experienced both conditions four times and were forced left for half their forcing series and right for the other half. The first arm entered and total entries of the alternate, previously blocked arm were recorded for each subject during each series of free choices.

Results and Discussion

The total number of first choice alternations in the four series of free choices in each condition were counted for individual subjects. Also, for each free-choice series in the two conditions every hamster's total entries of the previously blocked arm was determined. From these latter totals, representative individual scores were arrived at by calculating medians of the four free-choice series in each condition for each animal. All scores were then compared between conditions by means of Wilcoxon matched-pairs signed-ranks tests (see Table I).

TABLE I

Medians of the two-response measures following two-change conditions, and results of Wilcoxon tests

Measure	Change conditions		T	N	z
	G	B/W			
Total first-choice alternations	3·00	3·00	6·0	6	0·42
Median entries	2·75	3·00	10·5	10	1·73*

†$P<0.05$.

On the first runs of the four free choice series 10 out of 12 hamsters in condition G and 12 out of 12 in condition B/W chose the previously blocked arm more than 50 per cent of the time. According to two-tailed binomial tests these numbers were significantly greater than chance expectancies (G, $P=0.038$; B/W, $P<0.002$). Likewise, the median entries of the previously blocked arm were greater than chance for 10 and 12 subjects in conditions G and B/W, respectively.

Although there was a significant effect in the direction predicted on *median entries*, this effect would seem to be slight in view of the lack of a significant difference between the two conditions on *total first choice alternations*. Nevertheless, it can be concluded that the hamsters showed on successive free choices a greater preference for entering the arm that had been previously blocked by a black or white door, i.e. the arm in which the greater brightness change had resulted from intertrial removal of the door. This would appear to support Dember's (1961) explanation for the effects of forcing. However, it is also possible that the greater preference for the previously blocked arm in the B/W condition was merely a carry over from the forced to the free trials of preference for the barrier and hence arm that was different in brightness from the rest of the apparatus. Further experimentation could determine which of these two interpretations is the case.

The authors are grateful to Mr. Trevor Williamson who ran the animals.

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REVERSAL LEARNING OF OBJECT AND POSITIONAL DISCRIMINATIONS BY MINK, FERRETS AND SKUNKS

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Mink, ferrets and skunks were tested on successive reversals of two-choice object or positional discriminations in a modified Wisconsin General Training Apparatus. All species showed considerable improvement in performance after considerable training and some subjects exhibited one-trial reversal learning. Mink and ferrets persistently made more errors when position cues were relevant than when object cues were relevant; the reverse was true of skunks.

Introduction

Improvement in learning repeated reversals of the same discrimination has been observed in a number of mammals as well as in birds and invertebrates (Harlow, 1949; Dufort, Guttman and Kimble, 1954; Cronholm, Warren and Hara, 1960; Warren and Warren, 1962; Kirk and Bitterman, 1963). Performance of primates on repeated reversals of non-spatial discriminations does not differ consistently from that of non-primate mammals or birds (Schusterman, 1962; Warren, 1965; Rollin, 1963; Hughes, 1964; Bitterman, 1963; Bacon, Warren and Schein, 1962). All these groups made progressively fewer errors on repeated reversal trials and they ultimately learned to reverse their responses very rapidly.

In the present study, repeated reversal learning of a single discrimination was observed in certain Mustelidae (weasel) family subjects, namely mink, ferrets and skunks. These species have not been systematically studied under laboratory learning conditions. Performance on spatial and non-spatial visual discriminations was compared to determine whether weasel-family subjects respond differentially to the two types of cues involved. Previous data for cats and monkeys indicate that cats learn spatial discriminations more rapidly than object-quality discriminations, while the reverse is true for rhesus monkeys (Harlow, 1951; Warren, 1965).

Method

The subjects consisted of eight animals of each species of mink, ferrets and skunks. About half the subjects in each species group were males. All animals were Carnivora order, Mustelidae family. Descented skunks were *Mephitis* species, or striped variety, commonly called Illinois skunks, and weighed 12 to 14 lb. prior to this experiment. Ferrets were a commercially developed species, *Mustela furo*, were brown or white in colour, and weighed

7 to 10 lb. Mink were also a commercially developed species; the Mustela vision, pearl variety, and ranged in weight from 6 to 8 lb. Animals ranged in age from 10 to 12 months.

Subjects were experimentally naive and were raised from infancy in the College animal colony. They were maintained on canned cat and dog food supplemented with vitamins, brewer's yeast and fish flour. Animals were housed in 17 × 36 × 17 in. wire cages. Whenever possible, subjects were handled or were permitted to move freely around the College laboratory. These procedures resulted in considerable taming of most animals except mink. The latter showed no tendency toward docility and were transported to the learning apparatus in a commercial self-trapping cage. Other animals were carried by the experimenter.

The apparatus was a modified Wisconsin General Test Apparatus (Harlow, 1949) consisting of two unpainted wooden chambers completely enclosed within a $\frac{1}{2}$ in. plywood container. One chamber, the start box, was 20 × 10 × 20 in. overall with the rear wall serving as an entrance. A 20 × 10 × 20 in. test chamber was separated from the start box by a 20 × 20 in. wall. Centred in the wall 5 in. above the floor was a 10 × 4 in. sliding door which permitted the subject access to the test chamber. A 20 × 7 in. one-way mirror through which the experimenter viewed the subject was located on the outside test chamber wall opposite the sliding door. The test chamber was illuminated by a 6 W. fluorescent light placed in the ceiling over the mirror. Below the mirror was a 20 × 20 in. door through which a sliding tray was passed by the experimenter. The tray served as a platform to support two 3 × 6 × 3 in. alleys, each equipped with a 3 × 3 in. door, painted in flat black, hinged at the top and opening inward to each alley.

The test objects consisted of a triangle, the apex of which was oriented vertically, and a square. The objects were wooden, and were 2 sq. in. in area and $1\frac{1}{2}$ in. thick. Objects were painted white and were fastened to the alley doors to prevent subjects from removing them.

Initial discrimination training

One-half of subjects within each species group were randomly assigned to a positional or to an object discrimination problem. For one-half the subjects in the object discrimination group, choice of the triangle was always rewarded; for remaining animals, choice of the square was reinforced. The right-left position of the rewarded object was varied in a balanced, irregular sequence. One-half of subjects in the position discrimination group were required to select the right alley door; remaining subjects had to select the left alley door to obtain the food reward which was a $\frac{1}{2}$ in. cube of meat located in the alley behind the correct door. Spatial position of objects was varied from trial to trial in a balanced, irregular sequence. Thus, irrelevant object cues were present on the positional discrimination. Training was continued until the subject attained a criterion of 10 consecutive correct responses.

Reversal training

After the subject met this criterion, reversal trials were begun and the subject was required to select the previously incorrect object or door. Subsequently the two objects or doors became alternately correct and incorrect as the above criterion was met. The non-correction method was used throughout testing.

Animals were trained on a series of 60 reversals. They performed 1 reversal a day, 7 days a week. Subjects were returned to their home cages after the last trial on each day and were immediately fed daily rations. Animals were maintained at 80 per cent of normal body weight.

Results

Initial discrimination learning

Median errors to criterion in initial discrimination learning for subjects trained on object or positional discriminations are presented in Table I. Mink and ferrets

TABLE I

Median errors in initial discrimination learning by mink, ferrets and skunks

Subjects	Discrimination		*U*
	Object	Position	
Mink	7·5	21·2	0†
Ferrets	10·3	25·4	1‡
Skunks	36·7	10·4	0

† $P < 0{\cdot}028$, two-tailed test.
‡ $P < 0{\cdot}058$, two-tailed test.

made significantly fewer errors on the object discrimination while the reverse was true of skunks.

Reversal learning

Median errors to criterion, plotted against successive blocks of ten reversals are summarized for each species separately in Figure 1. Errors to criterion on the first and last ten reversals for subjects of each species attempting each type of

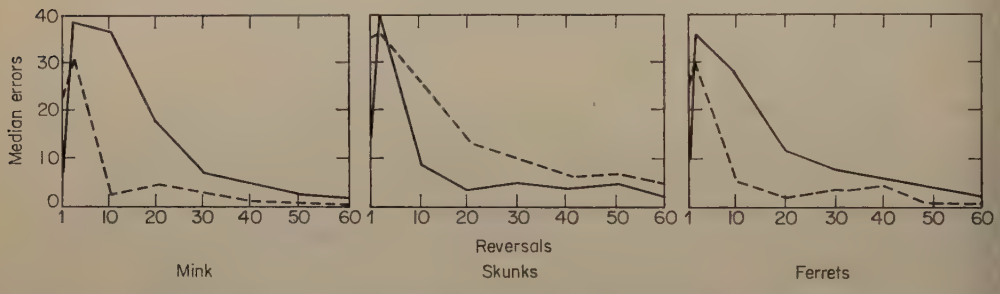

FIGURE 1. Errors to criterion on repeated reversals of an object and positional discrimination by mink, ferrets and skunks. – – –, Object discrimination; ——, positional discrimination.

discrimination were analysed by Wilcoxon matched-pairs signed ranks tests (Siegel, 1956). Performance of all species on both object and positional discrimination reversals differed significantly ($P < 0{\cdot}01$) on the initial and final ten reversals. All species showed considerable improvement in performing repeated reversals of both spatial and non-spatial discriminations. Mink attempting the object discrimination reversal showed the most rapid improvement.

Total errors to criterion on object and position reversals were analysed by separate Mann-Whitney *U* Tests for each species (Siegel, 1956). Significant intra-species differences were found in performance of all three species groups on the two types of reversals. Mink and ferrets made significantly ($U = 0$, $P < 0{\cdot}028$, two-tailed test) fewer errors on object discrimination than on positional discrimination reversals. Skunks learned to reverse the positional discrimination more rapidly ($U = 1$, $P < 0{\cdot}05$, two-tailed test) than the object discrimination.

The percent of correct responses made by the three species groups on trial 1 to 10 of reversals 1 through 10 and reversals 50 through 60 are presented in Figure 2.

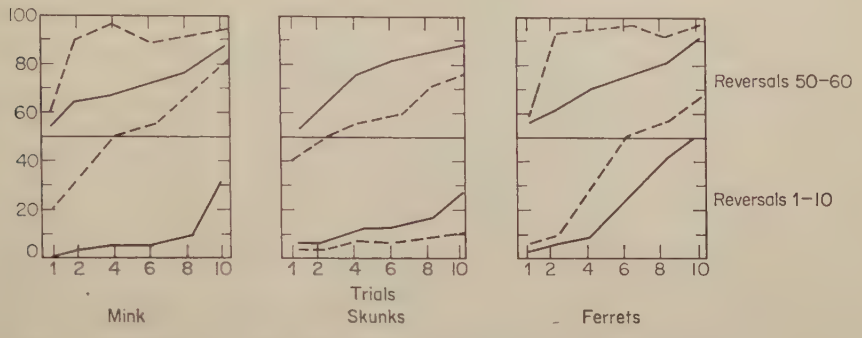

FIGURE 2. Intrareversal learning by mink, ferrets and skunks.

This figure indicates that initial performance on both types of reversals was poor among all species. However, some subjects, namely mink and ferrets, attempting the object discrimination reversal, and skunks trained on the positional discrimination reversal task, exhibited one-trial learning after repeated reversal training. The initial intra-species differences on the two types of reversals persisted to a reduced degree throughout training.

Discussion

It is apparent that certain weasel-family mammals successfully perform repeated reversals of object and positional discriminations with a single pair of stimuli. The discrimination reversal learning curves exhibited by mustelidae species are similar to those noted in a number of other species including primates (Schusterman, 1964; Warren, 1965). These findings suggest that quantitative rather than qualitative differences characterize mammalian discrimination-reversal learning.

Substantial intra-group differences appeared between object and positional discrimination reversal performance. Mink and ferrets approached optimum performance on the object discrimination reversal more rapidly than did skunks. Conversely, skunks learned to perform repeated reversals of the positional reversal more rapidly than did other subjects. Other researchers have found that cats have difficulty learning non-spatial discriminations (Warren, 1965). Skunks, like cats, may be more responsive to position than to object cues. Performance of ferrets and mink on the two types of discriminations suggests that these species, like certain primates, respond more strongly to object than to positional cues (Harlow, 1951; Warren, 1965, 1966). However, evidence summarized by Warren (1965) indicates that species differences in responsiveness to object and positional cues are not closely related to differences in complex learning ability.

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VISUAL COMPLEXITY AND STIMULUS PACING IN CHICKS

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Two groups were tested for spontaneous alternation using massed and spaced trials but alternation rate was not above chance level. Two experiments then examined the use of visual stimuli as pacers for an approach response. The results indicate that subjects responded to complexity and that complexity may be a more effective pacer than novelty.

Introduction

Numerous studies have examined aspects of the relation between stimulus complexity and behaviour in the rat, under a variety of conditions. The common finding of such studies is that the rat will show a preference for the more complex of two stimuli, for example, Dember, Earl and Paradise (1957) and Baker and Franken (1967).

This research suggests that some characteristics of visual stimuli can be used to elicit and pace an approach response in rats. Pace is used in the sense that some aspect of the stimulus has a directive function.

The purpose of the following experiments was to study the effectiveness of both complexity and novelty as pacers for a running response.

Method

Subjects

Subjects were experimentally naive male and female white leghorn chicks. Their age at the beginning of the experiments was 10 to 14 days.

Subjects were maintained in brooders with food and water *ad lib* on a 12 hr. light-dark cycle. The daylight cycle commenced at 8 a.m. and subjects were run in the apparatus commencing at 10 a.m. Thus no subjects came to the apparatus food deprived.

Apparatus

The apparatus consisted of a single straight runway 30 in. long, 6 in. wide, 8 in. high with an 8 in. × 8 in. goal box at each end. Each goal box was equipped with a Perspex guillotine door. Two additional Perspex guillotine doors were used to divide off the middle 6 in. of the runway so that it served as a start box. The subject was placed into the start box through a trap door in the roof of the runway. The walls and ceiling of the runway were lined with acoustic tiles to make it light proof and sound dampened. A 40 W. light was suspended 3 ft. above the centre of the runway so that both goal boxes were illuminated. The floor of the runway and the goal boxes was lined with brown paper, which was changed after each group of subjects had run one block of trials.

Procedure

Subjects were removed singly from the brooder and immediately placed in the start box; the start box doors were raised and the subject could retrace until it entered a goal box. The goal box door was then lowered and the subject left in the goal box for 3 sec. On massed trials the subject was then returned immediately to the start box and the next trial commenced. After each day's trials, or block of trials, the subject was placed in a covered cardboard chick-box lined with shredded paper and left there until all subjects in the group had been run. The order of running was randomized daily for each group. Subjects did not receive any primary reward in the goal box.

Experiment I

The purpose of this experiment was to establish whether there was any tendency for subjects to alternate when all cues (in the sense of Douglas, 1966) were present. Group I ($n = 10$) received four massed trials on one day; Group II ($n = 10$) received four trials with a 24-hr. intertrial interval. Subjects ran to grey goal boxes at each end.

Results and discussion

Group I (massed) alternated on 16 out of 30 trials; Group II (spaced) alternated on 18 out of 30 trials. Group I entered the right-hand goal box 21 times out of 40 trials; Group II entered the right-hand goal box 23 times out of 40 trials. Neither of these differences is statistically significant.

It is obvious that under these conditions chicks do not exhibit spontaneous alternation nor do they show any significant response bias. This supports the study of Hayes and Warren (1963) who failed to obtain spontaneous alteration in chicks using both free and forced choice methods.

Experiment II

Following Experiment I, a further experiment was designed to examine the possibility of pacing the running response in chicks with visual stimuli of differing complexity. The stimulus cards were 8 in. $\times$ 8 in. squares of gloss-white cardboard across which 1 in. wide strips of black plastic tape were attached in the following designs: (a) a single central horizontal strip having equal halves of white ground; (b) two horizontal strips giving equal thirds of white ground; (c) two horizontal strips and one central vertical strip giving equal sixths of white ground; (d) two horizontal and two vertical strips giving equal ninths of white ground. The cards were fitted to the end wall of the goal boxes as required.

Following Dember, Earl and Paradise (1957), stimulus complexity is defined as the number of elements constituting each stimulus. In our stimuli we have an increasing number of elements through designs (a) to (d), the assumption being that this corresponds to an increasing complexity of the visual stimulus.

Method

Twelve subjects were given four 24 hr. spaced trials. The position of the stimuli was randomized for each subject, with the restriction that the more complex stimulus appeared equally often in each goal box.

Stimuli were presented in order of increasing complexity so that, for example, on trial 1 the comparison was between a plain white card and stimulus (a), and on trial 4 between stimuli (c) and (d).

Results

Considered overall the more complex stimulus was chosen on 68·75 per cent of the trials.

TABLE I

Per cent subjects choosing more complex stimulus with corresponding P *values*

Trial	%	*P*
1	66·6	0·194
2	83·3	0·019
3	75·0	0·073
4	50·0	0·613

All *P* values are from Siegel (1956) for the Binomial test.

Any interpretation of the results is confused by the fact that on the first trial a choice must be made between two stimuli which are equal in novelty but not equal in complexity. In trials 2, 3 and 4 complexity is confounded with novelty in that the more complex stimulus is also the novel stimulus. Because of this it is not possible to say whether subjects are responding to novelty or complexity. Experiment III was conducted to examine this problem.

Experiment III

Method

Twenty-five subjects were given four 24 hr. spaced trials. Half the subjects always had the more complex of the two stimulus cards in the right goal box, for the other half this was reversed.

Stimuli were presented in order of decreasing complexity so that, for example, on trial 1 the comparison was between a plain white card and stimulus (d), on trial 2 between stimuli (d) and (c), and on trial 4 between stimuli (b) and (a). On trial 1 the stimuli were equal in novelty, but not in complexity, and on trials 2 to 4 the novel stimulus was the less complex.

Results

Considered overall the more complex stimulus was chosen on 64 per cent of the trials.

TABLE II

Per cent subjects choosing more complex stimulus with corresponding P *values*

Trial	%	*P*
1	76·0	0·007
2	68·0	0·054
3	56·0	0·345
4	56·0	0·345

Conclusions

On the basis of these results it is reasonable to conclude that an approach response by chicks can be elicited and paced with a complex stimulus. Although novelty is also an important factor it has less effect than complexity. Further experimentation is required to examine the relative significance of these two factors. For example, the length of time of exposure to the stimuli might be varied systematically, since it could be argued that with an exposure of 3 sec., a complex stimulus may still retain some novelty on later presentations.

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THE DEMONSTRATION OF DICHOPTIC FLICKER EFFECTS BY USE OF POLAROID

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A simple way of setting up dichoptic flicker with variable phase relationship is described. Sine wave flashes result, and for these, as for square waves, Sherrington's hypothesis of independent testing for intermittence of inputs to each eye is approximately borne out. Observations of the attributes of these two modes of stimulation at sub-fusion frequencies suggest that no such independence holds for computation of luminous flux over time.

The effect on flicker fusion of varying the phase relationship between flickering light sources presented at corresponding loci to each eye was explored by Sherrington (1904). He found critical fusion frequency to be only very slightly less for the anti-phase than for the in-phase condition. Subsequent work has confirmed a decrement of about 8 to 11 per cent due to anti-phase timing (Baker and Bott, 1951; Foley and Stager, 1965; Perrin, 1954; Thomas, 1955). The percept thus fails fully to reflect a major feature of the distribution over time of light incident upon the visual system. In the in-phase condition, in which the binocular input is synchronous, true intermittency of incident light exists. In the anti-phase condition the incidence of light is constant, varying only in its distribution between the retinae. It follows that decision as to intermittency of input is made prior to confluence of information entering though these two channels, with only a minor degree of cross-talk between them. Separate computations on either side are perhaps compared and averaged, or possibly there is alternation between the two eyes, as proposed by Hochberg (1964) for binocular vision in general.

The following simple device permits a classroom demonstration of these effects.

A small DC light source of variable intensity is mounted behind a rotating disc of polaroid. The disc is driven by a variable speed shunt wound DC motor, giving a speed range of 120 to 3000 rev./min. The subject wears a modified pair of B.O.C.‡ goggles in which the glass is replaced by matching polaroid discs mounted between thin Perspex. The orientation of one disc to the other is continuously variable between the in-phase and anti-phase condition of the sinusoidal input stimuli.

The stimulus frequency at either eye is twice the motor speed, which gives range of 4 to 100 c/s. To correct for any change in phase angle due to the variation

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‡ FGI goggles manufactured by British Oxygen Company Ltd.

in the facial contours of the subjects, the goggles are braced with 3/16ths steel rod. Provision is made for adjusting to different interocular distance.

The flashes differ from those previously used in that they take sine rather than square wave form. Critical flicker fusion frequency is slightly lower for sine than for square wave stimuli (Ives, 1922).

Observations with this apparatus have confirmed Sherrington's independence hypothesis for fusion threshold and a mean decrement of 12·66 per cent in critical flicker fusion frequency for anti-phase as compared in to-phase stimulation was obtained for four subjects. The findings may thus be generalized to the sine wave condition.

At a given sub-fusion repetition rate, in-phase and anti-phase flicker does not look identical. Anti-phase flicker looks brighter, and appears phenomenally faster. Attempts to adjust anti-phase flicker frequency to match these characteristics of in-phase flicker, used as a standard, gives rise to much variability, alerting the observer to the fact that the two conditions appear qualitatively different. While at least at low frequencies in-phase flicker appears punctuated by clear and discrete "black flashes", anti-phase flicker never actually gives the impression of clear interruption of light input, but only of a variability which at low frequencies takes the form of striking gamma movement. It is clear that at all times both eyes contribute to the ultimate percept.

Kahneman and Norman (1964) have given reason to suppose that separate neural systems respond to onset of a stimulus and to its overall intensity. If stimulus onset rate is computed prior to confluence of information from the two eyes and intensity subsequent to such confluence, these observations become intelligible.

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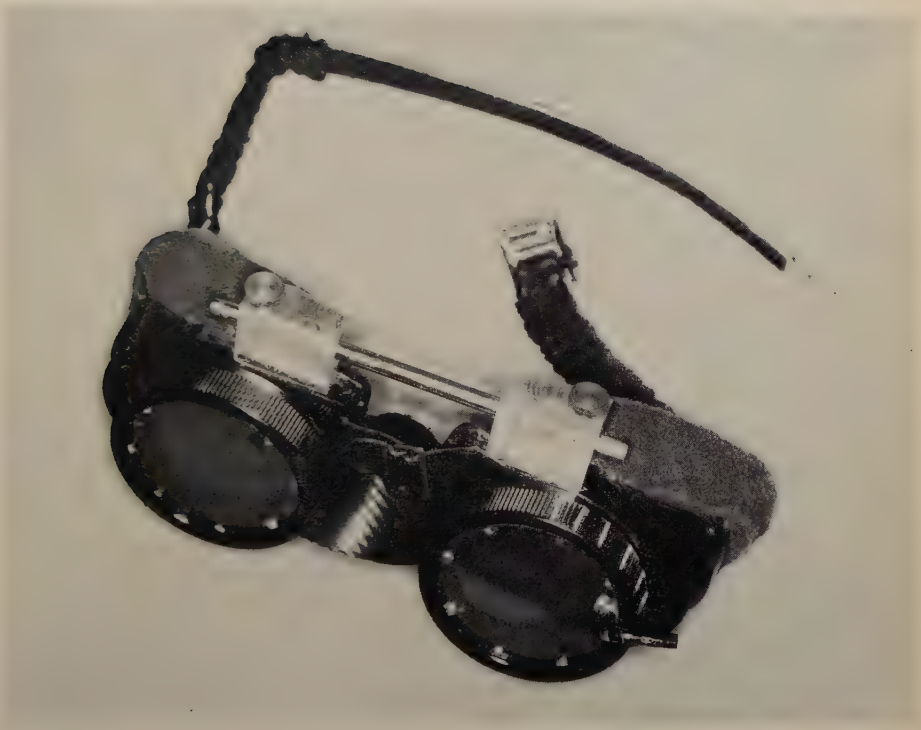

PLATE I. Sketch of modified goggles.

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DISCRIMINATION OF "W" AND "V" SHAPES BY GOLDFISH

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Goldfish were trained to discriminate between "W" and "V" shapes; different groups were trained with the shapes in different orientations. Transfer tests were given after training and the following conclusions were drawn. Animals learned to discriminate between the training shapes by detecting the difference in the number of points present in each; they learned the difference in the *relative* number of points rather than the *absolute* number present in each shape; the subjects transferred well to pairs of shapes bearing points facing in different directions from those on the training shapes; knobs were treated as practically equivalent to points; animals relied more heavily on differences at the tops of the shapes than on differences in the bottom halves.

Introduction

The experiment reported here is one of a series investigating visual shape recognition in the goldfish. There were two main reasons for choosing the training shapes used (depicted in row 1 of Fig. 1). (1) Octopuses find it almost impossible to discriminate between these shapes (Sutherland, 1959). The shapes differ only in their details, and octopuses are very poor at differentiating shapes whose main masses are roughly equally distributed but which have differences in local features. Other evidence suggests that goldfish are quick to discriminate between shapes of this sort (Bowman and Sutherland, 1969) and we tested this further by finding out whether goldfish would be able readily to discriminate between the present training shapes. (2) In the above experiment, it was found that the presence of a knob on a shape could mediate transfer to new rotations of that shape for goldfish, under conditions where such transfer did not occur without the knob. In the present experiment, we tested whether a difference in the number of points present in the training shapes would mediate transfer when further shapes were presented with points facing in different directions from those in the training shapes. It is of some general importance to establish under what conditions transfer to rotated shapes will occur, since very little is known about this.

Method

Subjects and apparatus

Full details and discussion of the apparatus and procedures used can be found in Mackintosh and Sutherland (1963), and in Bowman and Sutherland (1969). Only a brief summary will be given here.

The subjects were 18 common goldfish (*Carassius auratus*), from 3 to 5 in. in length. They were kept in individual tanks, at a temperature of 24°C.

The fish were trained to swim through a hole in a black Perspex detention screen, and take food from one of two troughs attached to the centres of the training shapes. The troughs and shapes were black and were presented on a white Perspex discrimination screen. The two screens were 12 in. apart, and the troughs were 5 in. apart and 2·5 in. from the floor of the tank. The troughs were separated from one another by a vertical Perspex partition projecting 6·5 cm. down the length of the tank. Each fish was trained in its own tank.

Pretraining

All fish were first pretrained to swim through the opening in the detention screen and take a food pellet from a single trough presented on one or other side of the discrimination screen. Each fish was trained until it was regularly taking the food within 10 sec.; this criterion was achieved in an average of 130 pretraining trials. Next, subjects were given 20 trials using the detention screen and a circle (area 17·5 cm.^{2}) behind the trough, to accustom them to the presence of a shape.

Training

Next, training proper was begun, with the positive and negative shapes presented simultaneously. Choice of the positive shape was reinforced with a piece of "Hykro" fish food; the inside of the trough attached to the negative shape contained a blob of paint resembling the food bait. Tests performed at the end of training proved that the fish had learned to discriminate in terms of differences between the two shapes and had not learned merely to discriminate between the food and the blob of paint. The side occupied by the positive shape was determined by Gellermann orders. The criterion of response was the fish's snapping at one of the shapes: this response is unmistakable and easy for the observer to categorize.

At first, if a fish chose wrongly, it was allowed to correct by swimming round to the positive shape. After 20 trials, by which time the fish had lost their initial nervousness of the situation, a modified correction procedure was introduced, in order to check the development of side preferences. When the fish made a wrong choice, the positive shape was shielded by a piece of transparent Perspex inserted between the vertical partition and the edge of the tank on the side of the positive shape. The subject was then re-run until it made a correct choice: if it chose the wrong shape on three successive runs, the negative shape was shielded by a transparent screen, forcing the fish to go to the positive shape. Throughout training, ten trials a day were given, with an intertrial interval of 15 min.

Control and transfer tests

After training was completed, control tests, in which both shapes were baited with food, were alternated with further training trials, in order to confirm that the fish had not learnt to discriminate merely on the basis of any perceptual difference between the food and the paint blob. No correction was given after an incorrect response in a control test.

Next we ran transfer tests alternated with training trials. On each transfer trial a pair of new shapes was shown, both baited with food.

Experimental design

There were three experimental groups, each of six fish; each group was trained to discriminate between a "W" and a "V" shape, but the orientation of the shapes was varied from one group to another. In what follows we shall refer to the training shapes as "W" and "V" shapes but it is important to remember that the shapes were shown in different orientations for different groups. For scale drawings of the shapes used with each group, see row 1 of Figure 1. The shapes were cut from $\frac{1}{4}$ in. black Perspex, and were 14·4 cm^2. in area. The trough was placed in the centre of each shape. Within each group half the fish were trained with the "W" shape positive, half with the "V" shape positive. The fish were given 200 training trials, including five control tests introduced on the last day of training.

All subjects were given transfer tests with pairs of new shapes. Twelve trials a day were given—six transfer tests and six retraining trials. The shapes used in the transfer tests are also shown in Figure 1. The tests were presented in three series, the first of twelve, the second of six, and the third of two transfer pairs. Within each series, the tests were given in random order; except in the third series, no pair was shown more than once within a day. Ten trials were run with each transfer pair.

Results

Initial learning

All goldfish met a criterion of at least 18 out of 20 correct responses over a two-day period by the end of training. There were no significant differences among the scores of the three groups, though Group III scored persistently lower than the other two groups, nor was there any difference between the scores of animals trained with "W" positive and those of animals trained with "V" positive. The average numbers of days taken to reach criterion by Groups I, II and III were 13, 11 and 14, respectively. On the twentieth day, the fish averaged 95 per cent correct on the control tests, compared with 93 per cent correct on the intervening retraining trials. The fish therefore, had learned to solve the problem by discriminating differences between the two shapes and had not merely learned to discriminate between food-pellet vs. paint-blob.

Transfer tests

The detailed results of the transfer tests are shown in Figure 1; throughout the retraining trials, the fish maintained a high level of performance (95 per cent correct, see row 1). The numbers in the figure show the percentage of transfer trials on which the appropriate member of the transfer pair was treated as equivalent to the originally-positive training shape. The scores are an average of the percent of trials on which the fish trained with "W" positive chose the left-hand member of the transfer pair (as drawn in Fig. 1) and on which the "V"-positive fish chose the righthand transfer shape. In the second and third transfer series, the Group I scores are based on only five animals, because of the death of one of the "W" positive fish.

The following statistics were calculated on the transfer test data. (1) The probability of all six fish scoring above chance in a given transfer test is 0·016 (0·032 where there were only five fish in the group): in Figure 1, stars are used to indicate these probabilities. Two stars indicate that all six subjects scored above chance, one star indicates that five subjects were above chance and the sixth scored at chance level, or in the case of Group I during Series II and III that all fish were above chance. (2) The significance of the difference between the scores made by the same group (or combination of groups) on two different transfer tests was evaluated by the Sign test. (3) The significance of the difference between the scores of two different groups on the same transfer test was evaluated by the Mann–Whitney U-test. As will be shown in what follows, the results of transfer tests are internally consistent and are therefore valid. Where the same transformation of the training shapes was presented in transfer tests, results for the different groups

agree. Where similar transformations of the training shapes were presented on separate tests to the same group, the results also agree.

A cursory glance at the transfer results suggests that animals had learnt the original discrimination by detecting a difference in the number of points present in

		GROUP I	GROUP II	GROUP III
		TRAINING SHAPES		
		Transfer Series I		
1	180° rotation of 'W'	92**	65	85**
2	180° rotation of 'V'	78**	60	78**
3	180° rotation of both shapes	88**	82**	78**
4	90° rotation of both shapes	73**	77**	80**
5	Same number of points at top or left of shapes	87**	85**	83**
6	Same number of points at base or right of shapes	57	55	77*
7	Same number of knobs at top or left of shapes	78**	80*	83**
8	Same number of knobs at base or right of shapes	55	55	77**
9		47	67	68*
10		63	72*	60*
11		62	65	55
12		55	62	50
		Transfer Series II		
13		68	75**	83**
14		76	73*	72*
15		84*	88**	77*
16		64	78**	82**
17		74*	82**	67
18		54	58	75*
		Transfer Series III		
19	Training vs. inverted 'W'	54	72*	58
20	Inverted vs. training 'W'	74*	58	53

FIGURE 1. Training and transfer shapes. The numbers indicate the percentage of trials on which the left-hand member of the pair of shapes shown opposite each number was treated as equivalent to the training "W," and the right-hand shape as equivalent to the training "V."

the two shapes. In describing the results of transfer tests we shall assume that this was true; however, since shapes differing in number of points differ in other aspects as well (e.g. total amount of contour present), more detailed arguments for supposing animals had detected a difference in the number of points present will given be later. The following main conclusions can be drawn from the transfer test results.

(i) *Top vs. bottom halves of shapes.* The animals' performance was determined by differences in the top halves of the shapes much more readily than by differences in their bottom halves. When shapes were presented having the same number of projections in their top halves as the originals, transfer for Groups I and II was very much better than when shapes having the same number of projections in their bottom halves were presented ($P < 0{\cdot}001$ for the significance of difference—compare test 5 with 6, and test 7 with 8). Group III, trained on shapes with points at the side, also did better in transfer when the shapes differed in number of points at the top than when they differed in number of points at the bottom (tests 16 and 17).

However, unlike Groups I and II, Group III did show some transfer ($P = 0{\cdot}062$) to shapes with differing numbers of points at the bottom, possibly because, in order to discriminate the training shapes, Group III was forced to pay attention to the *sides* of the shapes, whereas Groups I and II could concentrate solely on the top halves of the shapes. It is worth noting that early in training, Groups I and II tended to snap at the tops of the shapes, whereas Group III tended to snap at the sides.

(ii) *Position of points.* Given that transfer to shapes differing in number of points at the top was in general better than to shapes with different numbers of points at the bottom, it is necessary to ask whether transfer occurs to pairs of shapes with points facing in different directions from the points on the training shapes. Such transfer clearly did occur, since Groups I and II performed well with shapes differing in number of points at the sides, while Group III, as we have already seen, transferred when the differences occurred at the top or bottom (tests 4, 16, 17).

(iii) *Number of points.* The direction of transfer was determined more by the *relative* difference in the numbers of points than by the absolute number present in each shape. Groups I and II showed good transfer to any pair of shapes having different numbers of points at the top (tests 5, 13, 14, 15); the shape with the greater number of points was always treated as equivalent to the training "W," that with the lesser, to the "V." Transfer was excellent when the absolute numbers of points were the same as at the tops of the two training shapes (test 5); it was also good when the difference in the number of training points was exaggerated (test 15); transfer was significantly worse ($P < 0{\cdot}01$ for Groups I and II combined) when the absolute number of points on each shape was changed but the relative difference in numbers was the same as in the training shapes (tests 13, 14 compared with tests 5 and 15). In tests 13 and 14 we were pitting a difference in the relative number of points against any transfer due to one shape having the same absolute number of points as the original training shape: in test 13, the transfer shape with *fewer* points had the same absolute number of points as the training shape having *more* points at the top, whereas in test 14 the transfer shape with *more* points had the same absolute number

of points as were present at the top of the training shape with *fewer* points. Thus, in these two tests, it was the relative difference in number of points that determined transfer, but the fact that transfer was significantly reduced suggests that animals might also have learned something about the absolute number of points present at the tops of the training shapes. When shapes were shown with points at the sides, in positions corresponding to a 90° rotation of both shapes (tests 16, 17), transfer was again good, and was again determined by the relative difference in number of points.

These conclusions are supported by the results of transfer tests 1 to 3 with Groups I and II. Thus in Group I, when one or other training shape was rotated through 180°, transfer remained good provided that the "W" shape had more points at the top than the "V" shape (tests 1 and 3): there was less transfer (though the difference was not significant) when each member of the transfer pair had the same number of points at the top (test 2). Again, for Group II, transfer was poor when both shapes had two points at the top (test 1); transfer was significantly better ($P < 0{\cdot}02$) when one shape had two points, the other one (test 3). However, the results of test 2 with Group II are anomalous in that, when the "W" had three points at the top and the "V" only one, poor transfer occurred: we are inclined to attribute this single anomalous result to chance.

The results of tests 19 and 20 also support the above conclusions. In these tests one or other of the original shapes was presented with its own inversion. When the original "W" had more points at the top than its inversion (Group II, test 19) and when the original "V" had less points at the top than the inverted "V" (Group I, test 20) reasonably good transfer occurred. Performance was no better than chance when the original "W" had fewer points at the top than its inversion (Group I, test 19) and when the original "V" had more (Group II, test 20). However, when inversion of a shape changed the direction of difference in the number of points at the top in this way, we would have expected to find negative transfer, i.e. the inverted version of the training shape should have been treated as equivalent to the original "W" or "V." Since this did not occur, we must conclude that animals had learned something about certain characteristics of the original shapes other than the relative difference between the number of points at the top.

The results do not allow us to decide whether transfer for Group III was determined by the relative number of points present in a transfer pair rather than by the absolute number of points. The reason for this is that, whereas Groups I and II learned much more about the tops of the shapes than about their bottoms, Group III seems to have learned equally about both sides of the shape: thus, there was no significant difference in transfer to shapes having the same numbers of points as were present on the left-hand sides of the training shapes (test 5), and to shapes having the same numbers of points as the right-hand sides of the training shapes (test 6). While Group I learned to discriminate between two points and one, and Group II to discriminate between three points and two, Group III had the opportunity to learn both discriminations from the initial training shapes. The significant transfer obtained in test 18 with Group III suggests that they had learned on which side of each training shape two points appear: this conclusion is weakened by the results of tests 19 and 20, since no significant transfer was shown, although

within each pair one member has two points facing right, the other two points facing left.

(iv) *Knobs and points.* Where rectangles with different numbers of knobs were used, transfer was practically as good as to the corresponding rectangles with points (compare test 7 with 5 and test 8 with 6). This rules out the possibility that animals had learned to discriminate between the original shapes in terms of differences in the amount of *oblique* contour present in a particular area. It does not eliminate the possibility that animals were discriminating the difference in total amount of contour or the difference in the ratio $P/\sqrt{A}$, where P is periphery, A is area. This possibility is counterindicated by the results of test 9: although the outline parallelogram contains very much more contour in its top half than the filled-in parallelogram, there was little tendency to transfer from the original "W" to the outline parallelogram. Tests 10 and 11 show that there was some tendency to transfer to a pair of shapes, one of which had a thin segment orientated in the same direction as the points of the original shapes, the other of which had either a thick segment or no segment.

Discussion

One reason for performing the present experiment was to discover how easy it would be for goldfish to master the initial discrimination. Criterion was reached in about 130 trials: the discrimination is therefore more difficult for goldfish than that between a horizontal and vertical rectangle (45 trials—Mackintosh and Sutherland, 1963), and than a circle and square discrimination (85 trials—Bowman and Sutherland, 1969); the present discrimination problem takes almost exactly the same number of trials to master as the discrimination between two opposite oblique rectangles (135 trials—Mackintosh and Sutherland, 1963). The problem is much more readily solved by goldfish than by octopuses, since octopuses fail to discriminate "W" from "V" after 240 training trials, although they learn the horizontal/vertical discrimination in about 50 trials.

Elsewhere (Bowman and Sutherland, 1969) we have shown that goldfish may learn to discriminate between two shapes in terms of the presence vs. the absence of a knob, and we have shown that such a feature may mediate transfer to rotations of the same shape: fish trained to discriminate between a circle and a square showed no transfer to a 45° rotated square, whereas fish trained to discriminate between a circle and a square-with-a-knob showed excellent transfer when the square-with-knob was rotated through 45°. The present results confirm and extend that finding: they show that, where shapes differ in the *number* of points present, transfer will occur to other shapes having different numbers of points when the points are facing in new directions.

A further important finding is that transfer is determined by the relative number of points present in the transfer shapes more readily than by their absolute number of points. Transposition has frequently been found in animals on dimensions such as size and brightness, but as far as we are awared this is the first time it has been clearly shown with differences in shape. The results imply that goldfish can store a highly abstract rule describing an input shape; in the case of Group I, for instance,

the "W" shape must have been specified as (amongst other things) "the shape with the more points at the top" rather than as "the shape with two points at the top."

The tendency for animals to rely on differences in the top halves of shapes more heavily than on differences in the bottom halves again confirms earlier results of our own (see Bowman and Sutherland, 1969, for discussion) and also some results obtained by Hemmings (1965) on tropical fish.

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DISCRIMINATION OF CHECKERBOARD PATTERNS BY RATS

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Fifteen rats were trained to discriminate between a regular checkerboard pattern and a similar pattern containing a "mistake." The animals subsequently transferred to many other pairs of regular and irregular patterns differing widely from the originals. The results suggest that rats can induce and store highly abstract rules describing input patterns.

Introduction

Sutherland (1968) has recently argued that visual pattern recognition in vertebrates involves two separate processes. (1) The input picture is first decomposed into local features such as edges and corners: the physiological mechanism for detecting such features is known to be present in the vertebrate visual system (Hubel and Wiesel, 1965). The brain preserves the spatial relationships of these local features since there is a topological mapping of the retina onto different levels of feature detectors. (2) The brain stores a description of the feature detectors activated and their spatial relationships. The stored description must be sufficiently abstract to account for the known facts of shape equivalence. That is to say the description must be free from retinal position, and absolute size: the same description must encompass the original input shape, some of its perspective transformations and the mirror-image transform. The rule placed in storage must in fact describe not merely the input shape but a whole class of shapes of which the input shape is a member.

The description forming mechanism almost certainly has built-in biases. In particular, it is likely to take advantage of some types of redundancy in the picture so that a shorter rule will be stored than could be stored by listing independently all the feature detectors fired by a given picture and their spatial relationships to one another. Man can clearly form very abstract rules describing pictures: for example, most adults would tend to classify patterns 1–15 in Figure 1 as similar to one another, and Pattern Reg. as different. The stored description of a regular checkerboard pattern must be highly abstract—we can recognize a checkerboard pattern as a checkerboard regardless of the number of elements, the size of the elements, or whether the pattern starts in the top left-hand corner with a black or white square. Some psychologists (e.g. Glanzer, 1964) have supposed that the human ability to form abstract descriptions of pictures depends upon our possession of a natural language. There are two main advantages in storing an abstract

description of a pattern rather than in storing all the details. First, if the abstract description is correctly chosen, it will enable us to recognize an object despite changes in the proximal stimulus. Second, a considerable economy of storage space could be achieved by storing an abstract rule to describe an input picture rather than by listing all the feature detectors fired by that picture. Since these advantages apply as much to other higher animals as to humans, it is possible that they also form highly abstract descriptions of patterns and that this ability in humans is not dependent on language.

The present experiment was undertaken in order to test this possibility. We trained rats to discriminate between a regular checkerboard pattern and a similar pattern containing one irregularity, and then tested them with further patterns to discover to what extent they had learned a general rule describing regular checkerboard patterns. The experiment must be regarded as a pilot study for two reasons. First, as a result of experimental error a mistake was made in choosing the initial patterns to be discriminated. Second, it was difficult to know what transfer patterns to use in advance of gaining some knowledge of rats' performance in this type of discrimination so that after training various arbitrary decisions were taken by the experimenters as to how to proceed next. Though the results are not necessarily completely conclusive, they are thought to be sufficiently new and suggestive to be worth reporting at this stage.

Method

Subjects and apparatus

The subjects were 15 male hooded rats about 100 days old. They were maintained throughout the experiment on a 24 hr. feeding schedule, receiving 1·5 hr. *ad libitum* food after they had been run each day. They were trained on a Lashley jumping stand modified by the inclusion of landing platforms in front of each stimulus window (for fuller description of the apparatus and general procedures see Sutherland and Holgate, 1966). The stimuli used are shown in Figures 1 and 2; they were matt prints measuring 14 × 14 cm. pasted onto the doors of the jumping stand and covering the whole area of the doors.

Procedure

Rats were pretrained to jump to open doors. On training trials, they were rewarded with 10 sec. access to wet mash when they jumped to the positive stimulus. If they jumped to the negative, they were left on the landing platform for 5 sec. and then re-run until they selected the correct stimulus (modified correction procedure). After training, animals were given transfer tests on which at least one of the two patterns presented differed from the training patterns. On transfer trials animals were rewarded whichever stimulus they chose. Transfer trials were always alternated with retraining trials on which the training patterns were presented and animals were only rewarded for correct choices. The initial training shapes were those labelled Reg. and Irreg. in Figure 1. Seven rats (Group Reg.) were trained with Reg. positive (an eighth died), and eight (Group Irreg.) with Irreg. positive. We had intended to use the mirror-image of shape Reg. so that shapes Reg. and Irreg. would have identical elements except at two points where a black and white square are transposed. Our mistake demonstrates the difficulty humans have in discriminating mirror images.

The sequence in which transfer tests and retraining trials were administered will be described in the next section. Where probability levels are given either for differences of animals' scores from chance expectation or for differences between scores on different transfer tests, we applied a sign test unless otherwise stated.

Results

Stage I—Training

All animals were trained with shapes Reg. and Irreg. to a criterion of 18 out of 20 jumps correct over two days of training. Group Reg. met this criterion in an average of 137 trials, and Group Irreg. in 113 trials. The difference is not significant. In this apparatus it takes animals about 70 trials to learn to discriminate between a horizontal and vertical rectangle measuring 10 × 2·5 cm. Since this is known to be a very easy discrimination, the present shapes were moderately easy to discriminate.

Stage II—Initial transfer tests

All animals were given two transfer tests with each of shapes 1–15 (see Fig. 1), paired with the original Reg. pattern: each of these patterns contains a different "mistake," i.e. a different departure from a regular checkerboard. Ten transfer and ten retraining trials were given per day; the transfer patterns were presented in a random order. Group Irreg. performed at chance (50·3 per cent choices of shape Reg.) during transfer tests; Group Reg. chose shape Reg. on 72 per cent of tests—their performance was significantly above chance ($P < 0{\cdot}01$ by a t-test). During the retraining trials with the original shapes Group Irreg. were correct on 86 per cent of trials, Group Reg. on 94 per cent. Five of the seven animals in Group Reg. scored 23 out of 30 correct or better on transfer tests: such a score differs from chance with a probability of less than 0·01 by a one-tailed binomial test. These animals scored 97 per cent on patterns 1, 4 and 5 (containing an upright or inverted "T" shape), 78 per cent on patterns 2, 3, 6 and 7 (containing a "T" on its side), and 77 per cent on the remaining figures. If we break up the transfer patterns according to the lowest line in which a displaced element occurred, then animals averaged 97 per cent when there was a "mistake" in the bottom line, and between 78 to 80 per cent for the three other cases ("mistake" on second line from bottom, third and fourth). Although the differences are not quite significant, there is therefore a tendency for transfer to be better when an upright or inverted "T" figure appears in the irregular pattern, and when the mistake occurs towards the bottom of the pattern. Most of the remaining animals developed strong position habits during transfer tests, and Group Irreg. performed poorly even on retraining trials. It was therefore decided to retrain all subjects.

Stage III—Retraining

All animals were retrained using Pattern Reg. paired with Pattern Irreg. and the three 90° rotations of it (Patterns 1, 2 and 3). Pattern Reg. remained as the positive shape for Group Reg., and the four irregular patterns were positive for Group Irreg. Twelve trials were given per day (three with each of the four irregular patterns used) for 28 days. Over the first 24 trials of retraining, animals of Group Reg. averaged 68 per cent correct and of Group Irreg. 54 per cent correct; over the last 24 trials the corresponding figures were 83 and 68 per cent ($P < 0{\cdot}01$ for probability of this improvement being due to chance). Over the whole retraining period animals performed as follows: Shape Irreg.—89 per cent, Pattern

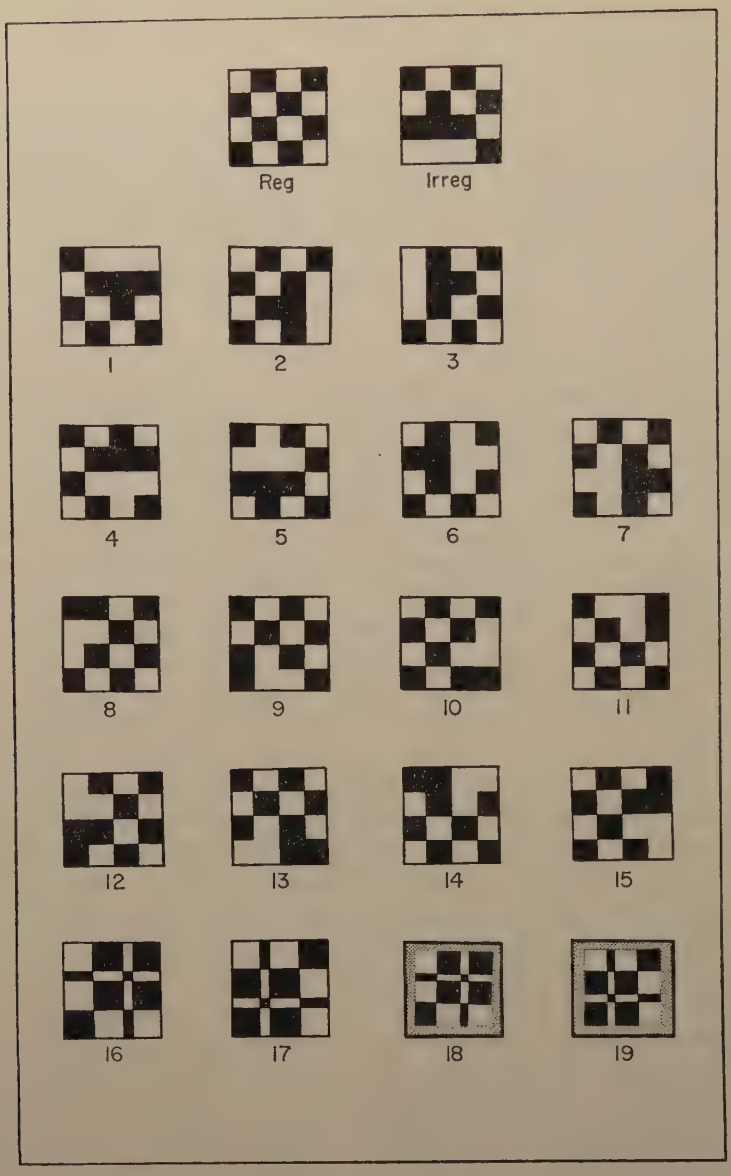

FIGURE 1. Training patterns and initial transfer patterns.

1—73 per cent, Pattern 2—63 per cent, Pattern 3—56 per cent. Performance was significantly better on Pattern Irreg. than on any other ($P < 0{\cdot}001$); it was also significantly better when an upright "T" appeared in the top part of the figure than when a "T" on its side appeared (Patterns 2 and 3, $P < 0{\cdot}001$). These results confirm those from Stage II. Animals performed better when the "mistake" occurred towards the bottom of the irregular pattern and when the irregular pattern contained an upright or inverted "T" rather than a "T" on its side.

Stage IV

At this stage animals were given further transfer tests on patterns 4 to 15 paired with Pattern Reg.: three trials were given to each animal on each pattern interspersed with retraining trials on Patterns Reg. and Irreg. All animals scored above chance during the transfer tests. Group Reg. again performed better on transfer tests than Group Irreg. (84 per cent correct vs. 74 per cent). Once again performance was better when upright and inverted "T" shapes appeared in the irregular patterns (Pattern 4 and 5, performance 89 per cent) than when a "T" on its side appeared (Patterns 6 and 7, 64 per cent, $P < 0{\cdot}002$ for the significance of the difference). Performance on the shapes containing "L"s (shapes 8–15, 80 per cent) was also significantly worse ($P < 0{\cdot}05$) than on shapes 4 and 5. Animals scored 83 per cent correct on patterns where the lowest mistake occurred on the first or second row from the bottom, and 69 per cent correct where the mistake occurred on rows three and four from the bottom—the difference was significant at better than the 0·001 level.

Stage V

In all the transfer pairs described so far, the irregular patterns contain somewhere within them a larger continuous area of black (and white) than do the regular patterns. To discover whether animals were merely discriminating between the two shapes in terms of the presence vs. the absence of such large continuous area of the same shade, further transfer tests were run with irregular Patterns 16 to 19 in Figure 1 each paired with Pattern Reg. These irregular patterns were made up by squashing one row and one column of elements. Since the total size of Patterns 16 and 17 is the same as that of the originals, the size of the square elements is larger: for this reason we included Patterns 18 and 19 which have squares of the same size as those in the training patterns. Twelve tests were run with each pattern.

During transfer tests animals averaged 72 per cent correct (all animals but one were above chance, $P < 0{\cdot}002$). There were no significant differences in amount of transfer to any of the four patterns, the scores on each ranging from 69 per cent to 74 per cent. Group Reg. again performed better than Group Irreg. both during retraining trials (95 per cent vs. 89 per cent) and during transfer tests (81 per cent vs. 64 per cent).

Stage VI

In all the tests reported so far, the original regular pattern has always appeared as one member of the pair. It could be argued that rats had simply stored an exact representation of this pattern and that transfer was being determined entirely by the presence of the original regular figure. To discover whether this was the case a series of further transfer tests was carried out in which both the regular and the irregular patterns differed from the original training patterns.

All animals were given 24 transfer tests with pairs 20 and 21 in Figure 2. One pair is made up of patterns containing 36 elements, the other of 64-element patterns; only 16 elements were present in the original training patterns. A further change was also made to the regular pattern: instead of starting with a white square in the

top left hand corner, the regular patterns used started with a black square in this corner. Animals scored 72 per cent ($P < 0{\cdot}003$ for difference from chance performance) on the 36-element patterns, and 58 per cent correct ($P < 0{\cdot}006$) on the 64-element patterns.

Stage VII

Since some rats were developing position habits in transfer tests and hence performing poorly, we decided to retain only the eight animals showing consistently good performance. Three of these eight were from Group Irreg. and five from Group Reg. Twenty-four transfer tests were given to each animal on each of seven new transfer pairs (numbers 22–28 in Fig. 2). On the interspersed retraining trials Group Reg. scored 98 per cent correct, Group Irreg. 96 per cent correct. There were no significant differences between the performance of the two groups on any of the transfer shapes used.

The transfer scores are shown in Figure 2. All animals scored above chance on all tests except tests 23 and 28. Animals did not score significantly above chance on these two tests but on the remaining five tests scores were very significantly above chance ($P < 0{\cdot}005$ on each).

Since all the regular patterns used differed widely from the original Pattern Reg., it is clear that transfer was not being effected by the animals having stored an exact representation of the original regular pattern. Transfer occurred when regular and irregular arrays of triangles and diamonds were presented (Pairs 23–28) and it occurred both when the irregularity introduced a larger continuous area of black and white when it introduced smaller elements. Transfer again tends to be better when the irregularity occurs at the bottom of the shape (compare performance on Patterns 23 and 24, and performance on Patterns 20 and 22). Nevertheless, good transfer can occur when the irregularity is not located at the bottom of the Pattern (25, 26 and 27).

Discussion

The experiment reported was undertaken in order to discover whether rats can learn an abstract rule describing a regular pattern. The transfer test results show that rats do form such rules and suggest that our own ability to discriminate between regular and irregular patterns is not dependent on language. The experiment is in a way a pilot experiment in which we moved from one stage to the next, guided by the results of the previous stage. Some of the results are not as clean cut as we would have liked, but it is felt that the experiment is worth reporting as it stands both because it opens up a new field of enquiry in pattern recognition by animals and because of the significance of the overall results of the transfer tests.

Faults in design

Our mistake in training animals on the mirror image of the regular pattern intended, makes the experiment untidy, but, fortunately we were able to control against the possibility that rats had detected the lack of correspondence between the *regular* elements in the two training shapes: the transfer results clearly show that rats

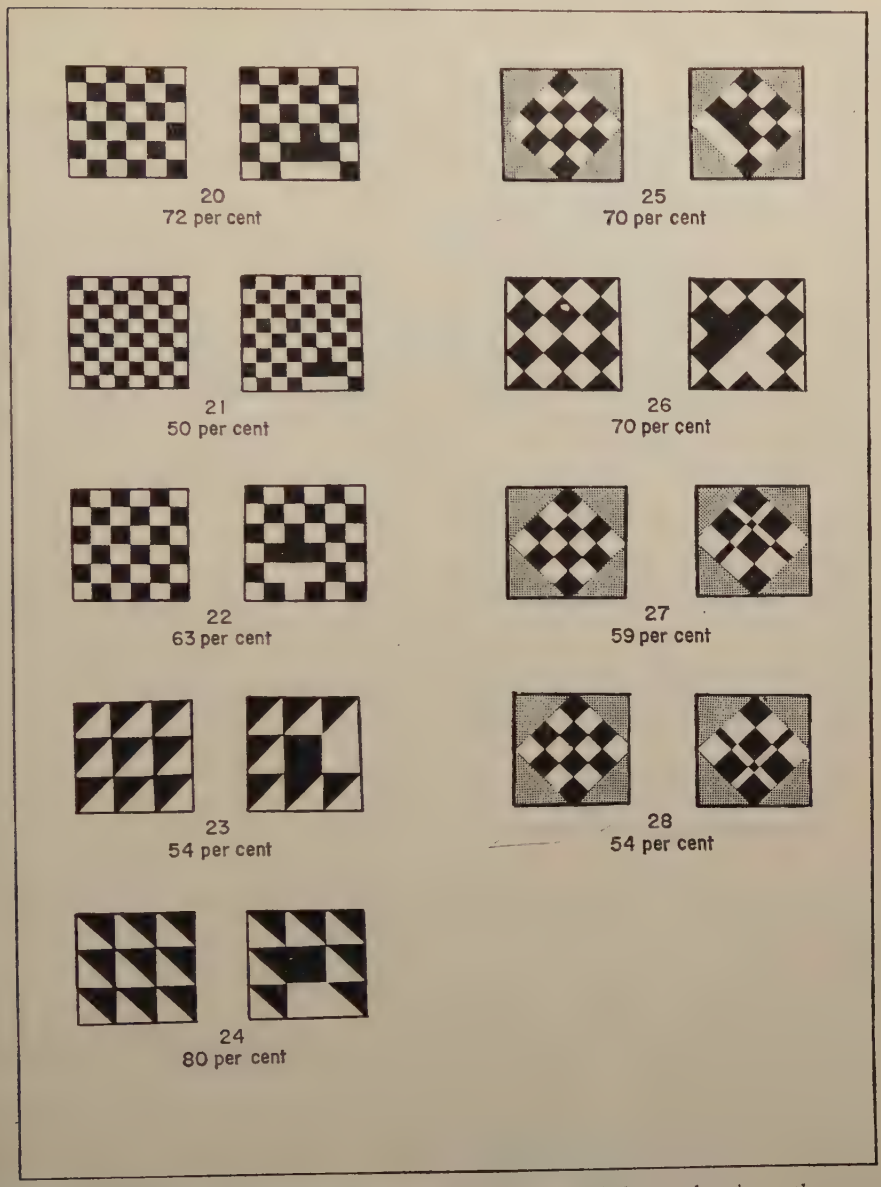

FIGURE 2. Further pairs of transfer patterns. The numbers below each pair are the percentage of correct responses made to that pair.

had not learned the discrimination in terms of the mirror image difference in the upper parts of shapes Reg. and Irreg.

The experiment leaves the following points unresolved.

(1) The subjects may have performed better when irregular shapes contained a mistake in the lower half than when the mistake occurred in the upper half either because the mistake in the training pattern Irreg. occurred in the lower half or

because of rats' well known tendency to detect differences in the lower halves of shapes more readily than in the upper halves.

(2) The subjects performed better when a three element horizontal bar appeared in the transfer shape than when a three-element vertical bar occurred: we do not know whether or not this was because a three element horizontal bar appeared in the original Irreg. training pattern.

(3) We cannot say for certain why Group Reg. showed significant transfer at Stage II while Group Irreg. did not. Since Group Irreg. were trained to jump towards Pattern Irreg. they may have stored more information about the actual locus of the irregularity and hence transferred less well when the locus was changed. Jenkins and Sainsbury (1968) showed that pigeons trained to peck at a pattern containing a feature and not to peck at a pattern lacking the feature, directed their pecks to the positive pattern at the feature. In the same way the rats of Group Irreg. may have orientated themselves to the irregularity.

(4) Finally, we are not in a position to state exactly what rule was formed by rats to describe the regular pattern. The transfer tests eliminate the possibility that the animals had learned to treat any pattern with a large continuous area of black or white as Pattern Irreg. They might, however, have learned that elements of two different sizes appeared on Pattern Irreg. and of only one size on Pattern Reg. though this is counterindicated by the good transfer that occurred with Pattern 26 in which the regular member of the pair contained elements of two different sizes.

Despite these unresolved problems, our experiment does demonstrate unequivocally the capacity of rats to store an abstract description of a pattern: they do not merely store a list of the feature detectors fired by an input picture.

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MEMORY WHILE SHADOWING

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Subjects were asked to repeat verbally (shadow) English words which were presented to one ear. They were then tested for their memory of two-digit numbers which were presented to their other ear. It is demonstrated that subjects have no memory for these digits if they must continue to shadow for 20 sec. before being tested on their memory for the digits. However, if tested immediately after the digit presentation, they do remember some digits. Hence, verbal material presented on non-attended channels gets into a short-term memory, but is not transferred into long-term memory.

Introduction

Current theories of attention agree that there is a limitation on our ability to perform several tasks simultaneously: they disagree about the location of the limitation. To simplify the argument somewhat, consider that selective attention operates by disconnecting or attenuating all channels of information arriving at the human except the one of primary interest. The question that arises concerns the level at which this selection takes place: does it occur before or after all channels have been analyzed for their content?

The question arises because signals on non-attended channels can sometimes attract attention if their meaning is important or relevant. Proponents of early selection (Broadbent, 1958; Treisman, 1960, 1964*a*, *b*; Treisman and Geffen, 1967) argue that selection among channels is based entirely on the physical properties of the sensory information and meaning is extracted only from those signals which are selected for further processing. Important signals can usually be detected regardless of the channel on which they occurred because the detection threshold for these signals is sufficiently low to compensate for the attenuation they receive when on non-selected channels. Proponents of late selection (Deutsch and Deutsch, 1963; Norman, 1968) argue that all incoming signals undergo a preliminary analysis of their contents: selection then takes place with the aid of both meaningful and physical properties. The critical distinction between these two classes of theories resides in the way that meaning is extracted from sensory signals. This distinction leads to a testable prediction. The meaning of a physical signal cannot be determined without reference to memory, for the meaning which we attach to the somewhat arbitrary visual and sound patterns of speech and writing must be stored in permanent memory. If meaning is extracted from all signals, then everything presented to us must get analysed through permanent memory and must also reside briefly in short-term memory. This theory must predict, therefore, that we can remember something about all events, even if we fail to pay attention to them. The present experimental literature argues against this possibility.

In general, we remember little of non-attended tasks. This result appears to have been stated first by Cherry (1953) and studied in greatest detail by Moray (1959). In particular, Moray asked subjects to listen to two channels of auditory information presented dichotically while verbally repeating (shadowing) the material heard on one channel. A list of common English words was repeated 35 times over the non-shadowed channel. Within 30 sec. of the end of the shadowing task (which continued for a while after the last presentation of the word list), the subjects were given a recognition memory test for words on the non-attended channel: the false recognition rate was about the same as (and slightly higher than) the correct recognition rate, implying absolutely no memory for the words. A useful control of his experimental procedure comes from the consideration that when the subject's own name was presented on the non-attended channel, it was often recognized.

Several other investigators have examined memory for events on the non-shadowed channel, but in the few cases where memory was found, it was possible to explain this by appealing to a possible or actual disruption of the shadowing task (Mowbray, 1962, 1964). (However, one hint that a brief memory may exist comes from the experiments of Peterson and Kroener, 1964.)

These results, though impressive, do not settle the issue. Under the theories of early selection, failure to remember material on the non-shadowed channel results from the selection occurring during the perceptual analysis of incoming information. To understand the predictions of the theories of late selection, we must first consider what processes are involved in the storage of information and, in particular, the interaction between short- and long-term memory

It is a common assumption that conscious attention is necessary to enact the transfer of material from short- to long-term memory (Waugh and Norman, 1965). If this is so, then we would expect that material on non-attended channels might get into short-term memory but, as long as the subject was occupied with the material on the attended channel, nothing would get into long-term. The data analysed by Waugh and Norman suggest that as few as six to ten items occurring between the presentation of an item and its test reduce the chance that it will later be recalled to zero. Accordingly to this view it would only take a few seconds of shadowing to erase short-term memory completely.

The purpose of this paper is to demonstrate that material presented to the non-shadowed channel is, indeed, stored in short-term memory, but that it never gets to long-term memory. It is not sufficient simply to demonstrate that it is possible to have a memory for items presented on the non-shadowed channel, for that might simply prove that the experiment was poorly done. It is also necessary to prove that there is no memory when conditions are made similar to those used by previous experimenters. Because the results and interpretation of these types of experiments are critically dependent on the experimental procedure, these details will be described in more detail than is usual.

Procedure

Subjects were asked to shadow material presented to one ear, making as few mistakes as possible. Simultaneously, a list of two-digit numbers was presented to their other ear. At a signal, the subjects stopped shadowing and immediately listened to the memory channel,

where a test number was presented. The subjects were to decide whether that test number had appeared in the preceding list of numbers. The details of the shadowing task were chosen to make it difficult to perform and the subject's performance was scored. The details of this task and of the scoring procedure were similar to those used by Mowbray (1964). The memory task was chosen to be one about which much is known, and was similar to the recognition experiments of Norman (1966) and Wickelgren and Norman (1966).

One important concern in these experiments is the accuracy of the subjects in shadowing the material presented to them. Some experimenters believe that subjects should produce errorless performance, for errors might indicate a lack of complete attention to the task. This point seems questionable, for if subjects are able to perform without errors, who is to say that their capabilities are being stressed to the fullest? Thus, it would seem wise to design a task that leads to some errors, even when subjects are working at their very best. But, it is important to get good baseline figures on shadowing ability when no other task is required. Then, we can tell if there is any disruption in shadowing performance when the memory task is added to the situation by seeing if there is any disruption of shadowing beyond the baseline established previously. Because of the critical nature of the shadowing performance in this experiment, it seems desirable to use well-trained subjects who have reached a stable level of performance on both memory and shadowing tasks when either is performed alone.

Conditions

Four experimental conditions were studied: shadowing (S), memory (M), memory-while-shadowing (MS), and memory-interference (MI). In condition S, subjects were asked to shadow only and to disregard the memory material. In condition M, subjects were asked to do the memory experiment only and to disregard the words they heard for the shadowing task. In condition MS, subjects were asked to perform both memory and shadowing tasks, but were urged to avoid errors in shadowing. Thus, by instruction and by frequent discussion of the number of errors they made while shadowing, subjects were forced to divide their attention unevenly, with most emphasis on the shadowing task. (The importance of this procedure was discovered during a pilot study in which the subject's performance on the memory task showed little decrement due to the addition of shadowing. His shadowing performance, however, was severely affected by the addition of the memory task. This type of result, similar to that of Mowbray (1964) gives no information relevant to the question asked in this paper.)

Three different sessions of memory material and three sessions of shadowing material were recorded on magnetic tapes making nine possible experimental combinations. During the experiments, the shadowing and memory tapes were played to the subjects over two professional quality tape recorders and earphones. Material to be shadowed was always presented to the left ear and the memory test to the right ear. The subjects sat in individual sound-insulated cubicles. They responded to the memory task by punching their answers onto IBM cards by means of a portable card punch and to the shadowing task by speaking into a microphone. When scheduling permitted, subjects were run at the same time. Each subject participated in a total of approximately 40 experimental sessions (including practice), each session lasted about 1 hr.

Memory task (M, MS, MI)

In the memory experiments, subjects listened to lists consisting of six two-digit numbers. The digits were spoken by a digital computer in a male voice so that the timing and pronunciation of the various conditions was accurately controlled.† Each digit lasted exactly

† The digits were made by taking recorded segments of the voice of Douwe Yntema, adjusting them to last exactly 200 msec., and converting the analogue voltage waveform to a digital representation at a sampling rate of 10,000 Hz with 9 bit accuracy. The digits were spoken by putting them out through a digital to analogue converter directly to the tape recorder and thence to earphones. Only the digits zero through seven were used; with the exception that these digits were not compressed in time, the procedure was excatly the same as used in Norman (1966, p. 372). I thank Douwe Yntema for making punched paper tapes of these digits available to me.

200 msec., the two digits of a number were separated by 100 msec., and the numbers were presented at the rate of one digit pair per second. There were two conditions studied: *delayed* and *immediate*. In the *immediate* condition, at the end of the digit list, a 1000 Hz tone (also computer controlled) was presented for 500 msec., followed immediately by a test pair of digits. The subject was asked to judge whether the test number had occurred in the previous list by answering either *yes* (the test number had occurred) or *no* (the test number had not occurred) and then stating his confidence in the accuracy of his response on a four point scale.

In the *delayed* condition, there was a 20-sec. delay between the end of the number list and the presentation of the tone and test number. All other aspects were the same.

Each session of the memory material contained 100 trials, divided into two parts of about 20 min. each. The first 5 trials of each part were used for practice (this fact was not told to the subjects) and so were not counted. Of the remaining 90 trials, 60 were for the *immediate* condition and 30 for the *delayed* condition. Of the 60 immediate tests, 40 were *old* test items (10 at each serial position tested) and the remaining 20 were *new* test items. Of the 30 *delayed* tests, 20 were *old* test items (5 for each serial position tested) and the remaining 10 were *new* test items. To shorten the duration of the experiment only four serial positions in the list were tested: postiions one, three, five and six. The ordering of the conditions and tests was determined by the pseudo-random number generator of the computer: the subject could only tell what condition was being tested at the time of the test itself.

Shadowing task (*S*, *MS*)

Each session of the shadowing material consisted of two parts of about 25 min. each, containing a continuous stream of about 3000 monosyllabic words spoken in a female voice at the rate of two words each second. The words were selected randomly from a list of 2000 words selected from the Thorndike-Lorge collection by Waugh (1961).

Subjects were trained to shadow accurately. They never knew when in a session their shadowing performance was under surveillance, but in fact (because only one subject was monitored at a time) only one-half of any subject's performance was scored. Scoring followed a rigid criterion: any deviation in pronunciation was scored as an error (we followed the rules described by Mowbray, 1964). Occasionally an independent scorer also evaluated the shadowing performance, with very high correlation to the experimenter's scores. Subjects were often shown the results of the shadowing analyses and were continually warned that they were making too many mistakes.

Joint tasks (*MS*)

When performing the two tasks simultaneously, subjects started shadowing the words as soon as they heard them. They were especially warned against increasing their error rate in shadowing when the memory digits were presented. They were instructed to continue shadowing until they heard the 1000 Hz tone in the ear which contained the memory digits. As soon as they heard the tone, they were to stop shadowing, listen to the test digits, and make their response. Immediately after making a response, they were to start shadowing again. There was enough time between lists so that the subjects could do about 5 sec. of shadowing before the start of each list. (The signal to stop shadowing was very effective: missed responses on the memory task were very rare.) This experimental procedure takes advantage of the fact that subjects seem always to detect a tone, regardless of what else they are doing (Cherry, 1953).

Memory-interference task (*MI*)

This task was added in order to study the effect of the shadowing task as pure interference to the memory of the digit lists. Subjects were asked to listen to the digits of the memory task without shadowing. When the six numbers of the digit list had finished, if a tone and test number appeared immediately, they were to respond as well as they could. Otherwise, they were to start shadowing immediately and continue during the 20 sec. that would then elapse between the end of the digit list and the presentation of the tone and test number. In condition MI, therefore, the shadowing acted solely as interference to the memory in the

delayed test condition, much like any other interfering task such as colour-naming or counting backwards.

Practice

Subjects practiced shadowing prior to the start of the experiment until their error rate had reached a stable, asymptotic value. This took about 18 hr. of practice, distributed over 9 days. Subjects did several sessions of the memory task prior to the actual experiment to get them used to that condition. In addition, as already noted, the first five memory trials at the start of the day and after each rest were considered to be practice and were not scored.

Subjects

Because of the extreme difficulty and amount of time required to train and test subjects, only two were used in this experiment. (A third subject was used in a pilot study.) They were both female undergraduate students at the University of California, San Diego, who were paid for their services.

Results

The analysis of the shadowing performance is shown in Figure 1. Approximately 10 sec. are required for the error rate while shadowing to rise to an asymptotic value.

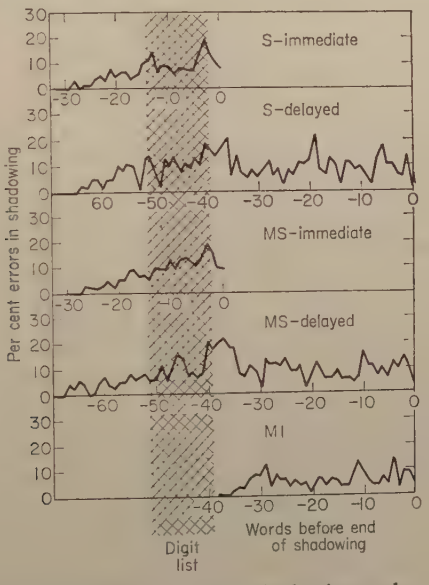

FIGURE 1. The analysis of performance on the shadowing task. The percentage errors are conditional upon shadowing taking place. Thus, the absence of a line indicates that subjects had stopped shadowing.

The shadowing performance, on the whole, is relatively independent of the memory task, although there is a noticeable increase in the error rate at the conclusion of the digit list.

It is not possible to compare the results from the various memory conditions directly because the false recognition rates varied widely. This is a typical result of experiments which use the so-called "yes—no" procedure, whether they be experiments in memory or in psychophysics (see, for example, Green and Swets.

1967; Norman and Wickelgren, 1965; Parks, 1966). For the present analysis, it is convenient to treat the memory task by the assumptions of strength theory (Wickelgren and Norman, 1966). The yes—no and confidence ratings of the subjects give us memory operating characteristics. From these, we can get estimates of memory trace strength for each item. (These estimates of trace strength for some item i, $t(i)$, are similar to the d' of signal detection theory.)

The data for the individual subjects were analysed separately. However, because there was essentially no difference between them, only the average results are reported here. The results of the trace strength analysis are shown in Figure 2.

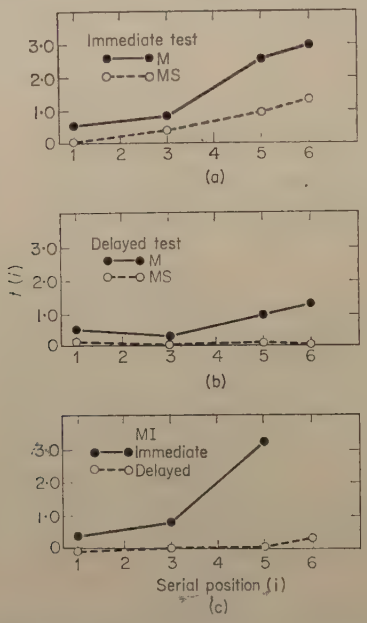

FIGURE 2. The comparison of memory trace strength, $t(i)$, with and without shadowing, conditions MS and M, respectively.

(a) Shows substantial short-term memory in both conditions. (b) Shows substantial long-term memory when the test is delayed 20 sec. after the end of the list but no shadowing is required (condition M, ———). When there is shadowing (condition MS), there is a negligible amount of long-term memory trace strength. (c) Shows that when shadowing takes place only in the interval between the end of the memory list and the test short-term memory (———) is the same as condition M of part A but long-term memory trace strength is completely negligible, as it is in condition MS of part B. (Trace strength for serial position 6 of the immediate test is too high to be estimated.)

The probabilities of correct recognitions, P(y| old), and false recognitions, P(y| new), are shown in Table 1.

Consider first the *immediate* tests of memory. Each estimate of correct recognition, P(y old), represents from 110 to 120 observations and each estimate of false recognition, P (y new), from 200 to 240 observations. It is clear that even though the *immediate* memory is not as good while shadowing (condition MS) as in the absence of shadowing (condition M)—memory strength for position 6 drops from 3·0 to 1·4—there is no question that subjects remember a substantial amount of material from both conditions (Fig. 2(a)). It is also clear that similar serial

position effects are observed in both cases. Now consider the *delayed* tests of memory. Each estimate of correct recognition represents from 50 to 60 observations and each estimate of false recognition from 112 to 120 observations. Although there is a substantial amount of memory and a serial position effect when memory is tested in the absence of shadowing (M), there is very little memory at all for the condition with shadowing (MS) (Fig. 2(b)). In condition MS, with *delayed* test, the correct recognition rate is just barely superior to the false recognition rate: the average trace strength of memory items is only 0·11—a very low value indeed. The conclusion one reaches, therefore, is that while shadowing there is a fair amount of *immediate* memory but that almost no memory remains after a delay of 20 sec.

TABLE I

Recognition probabilities in the memory tasks

Condition	Serial position				
	1	3	5	6	new
M-immediate	41	50	92	98	22
M-delayed	61	52	71	86	35
MS-immediate	48	59	83	83	48
MS-delayed	70	68	73	66	62
MI-immediate	43	58	98	100	24
MI-delayed	60	63	68	75	69

The short-term memory measured while shadowing (condition MS) is at a lower level than that observed in the absence of shadowing (condition M). There are at least two reasons for this. First, when the subject is shadowing, he is so busy that he cannot rehearse the digits, even though they are registered in short-term memory. Second, the subjects do not stop shadowing the instant the tone for the test digit is presented. As can be seen in Figure 1, subjects continued to speak for several words after the conclusion of the digit list. These two factors—the interference of the shadowing after the end of the digit list and the lack of rehearsal—both tend to lower the amount of material that is available in memory at the time of test, even if the digits themselves get in equally well whether or not shadowing is taking place. Indeed, the shadowing task is so complex, both in the skills required of the subject and the confusion of sounds that result, that the remarkable point is that there appears to be so little decrement of performance in the memory task. Peterson and Kroener (1964), for example, suggest that the confusion of voices may create a perceptual deficit.

Proof that the lack of memory in the MS-*delayed* condition results from the act of shadowing during the 20-sec. delay comes from a consideration of condition MI, where shadowing was performed only during the delay period (Fig. 2(c)). In the immediate test condition, each estimate of correct recognition represents 80 observations and each estimate of false recognition 160 observations; in the *delayed* test condition, each estimate of correct recognition represents 40 observations and each estimate of false recognition 80 observations. The results of MI-immediate are very similar to those of M-immediate condition, as well they should

be, for the conditions are identical. The results with *delayed* tests show slightly less memory for condition MI for condition MS (average memory strength is 0·07 compared with an average of 0·11 for condition MS) but a slightly greater serial position effect. These memory effects are minute, however. For example, the average probability of responding yes to an *old* item is 0·66: this is slightly *less* than the false recognition probability of 0·69. (The overall positive value for memory trace strength comes about through a consideration of confidence ratings.)

Discussion

While a subject is busy shadowing, he still retains a good short-term memory for other material which may be presented to him, but no long-term memory for that material. The experimental conditions used here which are most similar to those of previous studies is the *delayed* test of memory: the results are also very similar—little or no memory. The difficulty with the earlier studies appears to be in their neglect of short-term effects, which are now revealed to be rather large. This neglect is not surprising, for the earlier studies occurred before the recent extensive theoretical and experimental analyses of short-term memory. The present results must be interpreted to indicate that, although the act of shadowing prevents the subject from performing whatever operations (e.g. rehearsal) are necessary to retain material for reasonable durations of time, the short-term memory is still operative.

What do these results imply for theories of attention that assume that we turn off or attenuate all but a single channel of information? Accordingly to these theories, if full attention is paid to the shadowing task, the memory task of the present experiment must be "disconnected" or attenuated. The datum that must be explained by all theories is the short-term memory in the presence of divided attention.

As we have seen, one theory requires the existence of short-term memory. One way for other theories to handle this result is to postulate that there are sensory storage units prior to the attention mechanism. After the signal to stop shadowing has been presented, subjects are able to read out the contents of that sensory storage. When there is a long delay between the end of the memory list and the test, the material in the sensory store decays away. Such a sensory storage system plays an important role in Broadbent's early theories (1958). The main problem with this argument comes from the similarities of the properties of the sensory storage which would be required to handle the present results with the properties of the usual short-term memory. The characteristics of short-term memory usually seem to depend upon such non-sensory factors as the number of psychological units or "chunks" presented rather than upon such sensory factors as the duration of the material (Miller, 1956; Norman, 1966; Waugh and Norman, 1965). The quantitative similarity of the serial position curves from the short-term memory conditions of this study to those found in other studies (such as Norman, 1966 and Wickelgren and Norman, 1966) would appear to be too much of a coincidence to be explained by the parameters of a sensory memory. Further discussion of this point can be found in Norman (1968, 1969).

In conclusion, the experiments reported in this paper illustrate that even while

subjects are kept busy with a difficult shadowing task, they retain some short-term memory for other items presented to them. Little or no memory for these items remain after a lapse of 20 sec. The lack of long-term effects can be attributed entirely to the act of shadowing during the 20-sec. delay quite independent of the shadowing that took place at other times. These results are of critical importance for theories that postulate that all sensory signals undergo processing; a negative result would have ruled against these theories.

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BOOK REVIEWS

Brain Function. Vol. IV: *Brain Function and Learning.* Edited by D. B. Lindsley and A. A. Lumsdaine. Berkeley: University of California Press (London: Cambridge University Press). 1968. Pp. xiii + 364. 142s. 6d.

This is one of the conference series which records the discussion during and after a paper. I believe that the participants do some editing after the conference, which presumably makes for less liveliness and the two-year publication delay but also for less wasted print. Occasionally the earlier multidisciplinary conferences in this series managed an impressive few pages of truly *inter*disciplinary criticism and integration. Here the cross-disciplinary illumination glows rather than sparkles.

So much of the book's value must be sought in its achievement as a wide but coherent survey of undisciplinary work on the brain in learning. Its success is moderate. The papers are uneven in quality and somewhat disconnected, but the discussions rectify this to some extent. And, anyway, is that not the present state of research into the physiological mechanisms of learning?—different techniques being exploited in ways little related to each other and with varying relevance to the heart of the problem.

The neurochemistry and the electrophysiology of learning have a chapter each, thin on data and critical discussion of it. One could offer a newcomer to the biochemistry of memory a good brief introduction to the present problems, if he cared to work backwards through the discussion of Gaito. Galambos cites without exposition the attempts to find electrical correlates specific to learning by Adey and by John and does not mention the European results. An appendix to the following chapter describes R. F. Thompson and Spencer's analysis of the synaptic basis of spinal habituation in the cat.

There is only one other chapter on physical mechanisms mediating learning. Pribram gives a lucid, interesting introduction to the analysis of the behavioural effects of inferotemporal lesions up to 1964 and his ideas on the role of the "association" areas in corticofugal control of central brain stem sensory input, mediating selective attention and intention.

A number of the other chapters, which confine their data to brain function at the performance level, are also brief and clear introductions to important work. Konorski (with N. E. Miller and Pribram) expounds the experimental dissociation of classical conditioning and instrumental learning and a corresponding distinction in the food system between consummatory and search organizations. Lipsitt describes habituation and some form of conditioning seen in reasonably humane, but well-controlled, experiments with newborn human infants. The psychological, but *not* necessarily physiological, defect(s) in mental retardation are discussed by O'Connor with Zigler and Birch.

There is also Simon on computer programs as fully explicit theories of learning, Atkinson on algebraic models of verbal learning and Lumsdaine on teaching machine technology. Hilgard provides a liberal introduction to positions in qualitative learning theory. Throughout the book, incidentally, S-R theory is treated as a very live horse still enthusiastically backed by others, even though the participants of this conference place their bets elsewhere. Finally, there is Guildford on creativity and Koestler on the act of creation. It is well worth emphasizing that intellectual creation is the summit of human learning, but these authors do not succeed in being as representative of their area as do some other contributors.

As a collection of some good introductions to certain aspects of the analysis of brain functions in learning, which are not obtainable in quite this form elsewhere, the book should probably be in a medium-sized institutional library that included physiological psychology in its range. I cannot imagine an individual finding over £7 worth of usefulness in it on his personal bookshelf, but it could be worth an hour or two picking out the bits that interest (the Name Index includes discussion comments).

D. A. Booth

Lehrbuch der experimentellen Psychologie. Edited by R. Meili and H. Rohracher. 2nd edition, revised and enlarged. Bern and Stuttgart: Verlag Hans Huber. 1968. Pp. XII + 501. D.M.44.

It is a long time since a major text in experimental psychology emerged from its birthplace, Germany. This one, first published in 1963 and now reissued in a revised and enlarged edition, attempts to cover the gneeral field of human experimental psychology. The topics considered are psychophysics (Ekman), perception (I. Kohler), learning and memory (Rohracher), thinking (Meili), feeling and emotion (Karsten), personality (Brengelmann), experimental social psychology (Eyferth) and statistical methods (Pawlik). Oddly, perhaps, there is no chapter on language.

In spite of its many excellent qualities, this textbook is pervaded by a curiously old-fashioned sense of the "generalized normal adult human mind" which for Wundt and others of his time represented the subject-matter of experimental psychology. There will be found almost nothing about children or animals, about individual differences in ability or about brains and behaviour. Indeed Behaviourism might almost not have been and Gestalt psychology still maintains a ghostly presence. Clearly, little has changed in German academic psychology except its quality, which seems alas never to have recovered from the *debacle* of 1933.

None the less, one must welcome the Editor's initiative in compiling this text, which does make a good deal of modern work available to German-speaking students. It will no doubt be widely used and in spite of its limitations should keep the tattered banner of what was once the "new psychology" flying in the land of its birth.

O. L. Zangwill

The Measurement and Prediction of Judgement and Choice. By R. Darrell Bock and Lyle V. Jones. San Francisco: Holden-Day. 1968. Pp. 370. $13.75.

Forty years ago, L. L. Thurstone developed a system of psychological measurement based upon the notion that associated with each stimulus was a discriminal process which was a normally distributed random variable. Its dispersion was used to define the unit of a psychological scale. By assumption a person judges A greater than B, or chooses A in preference to B, if the discriminal value of A exceeds that of B. This minimal theory of the processes of judgement and choice led to models for sensory discrimination, comparative judgement and categorical judgement.

Bock and Jones dedicate their book to the master and give a systematic presentation of his models organized under the different methods for obtaining data—the constant method, method of pair comparisons, method of ranks and method of successive categories. Their major contribution, and it is an impressive one, is that they have brought together a wide variety of statistical techniques for solving the problems of parameter estimation, constructing confidence bounds and testing goodness of fit of each model examined. There can be few mathematical models in psychology for which so much attention has been paid to these problems.

For the basic scaling models and for a number of variants they compare graphical, minimum transform chi-square (Urban's method of weighted least squares) and maximum likelihood solutions to parameter estimation and also the confidence bounds on scale values that can be obtained in each case. They also construct appropriate analysis of variance designs for testing goodness of fit of each model.

This book should be useful to anyone concerned with extracting information from data, whether or not his interest is in scaling. He would be advised to first master some text in statistical theory at the level of, say, Mood and Graybill, although he is assisted by worked examples wherever new techniques are introduced. There are nearly 60 pages of tables and computing notes.

If the book is to be faulted we would point to the dissonance between the level of rigour established and the amount of computation this involves on the one hand and on the other hand, the importance of the psychological data discussed. The following detail is typical:

". . . Two samples of warmed green beans were served to customers, who tasted the samples and expressed their preference for one of them. . . ." (p. 141).

HENRY SHAFFER

Minnesota Symposia on Child Psychology. Vol. 1. Edited by John P. Hill. London: Oxford University Press, Minnesota University Press. 1967. Pp. 239. 47s. 6d.

In the last decade, most American work done in the name of child psychology involved taking ideas or techniques from mainstream psychology and trying them out on children; often it seemed, for no better reason than that children are there. Though some of the limits of generality of the adult and animal data became clearer, this era did not add much to our knowledge of development. Now there are signs of increasing concern for what must be the root of child psychology; the problems of development itself. Several influences have contributed to this change. One is certainly the growing knowledge of Piaget's work in the United States. The nativistic approach of the psycholinguists and their growing preoccupation with developmental questions has had its effects, too. Another strong push has come through the boom in studies of the neonatal period. Such research raises many developmental questions in their most acute form. From this perspective the volume under review is antedeluvian in outlook. With the exception of one chapter (by White), one may search in vain for any discussion of developmental problems.

The book has its origins in a symposium held by the Institute of Child Development of the University of Minnesota in 1966. Six speakers were "given the opportunity to discuss a series of related studies from the perspective of the theory or set of issues that guides their programs of research." This, we are told, has unique advantages for both the investigator and the reader, as it provides a "programmatic" perspective which is lost in any "second-person review" of an area and is fragmented in journal reports. The advantages seem a little less than unique as much of the work has been published elsewhere in very similar forms and for the most part the authors have used the opportunity to string together a number of papers with a little bland discussion, apparently unhindred by editing.

In the one example of work from the new wave, White discusses the sensorimotor development of institution children in the first year of life. He points out that we lack the basic observational data necessary to build or test theories of development. Though I feel he underestimates the amount of existing information, I would not quarrel with his plea for us "to sharpen our observational tools and to collect 20 years of solid natural history." He ascribes the dearth of observational work to the difficulties in getting hold of subjects, the lack of research time available for academics and the biases of journals against descriptive papers. These are superficial reasons. Children are easier to come by than say, adult bus drivers, and many animal workers, even in America, find time to do observational work. As for the journals, they merely reflect the work actually being done. The real reasons are slightly deeper than this and may be found in White's chapter itself. Having stressed the need for a biological and observational approach, he goes on to say that he sees the domain of the psychologist as the manipulation of experience through environmental modifications" and he "considers himself professionally obliged to work on the problem of how to structure a child's world so that experience has a maximum positive effect on growth and development," You can't have it both ways. In the long term these may be desirable ambitions, in the short term they are not going to get the observational work done. Natural history is not compatable with structuring the child's world.

This collaborative work (with Hayes, Held and Castle) shows that some environmental modifications influence the development of visual attention and grasping. Giving infants something interesting to look at results in earlier grasping, apparently because they spend less time looking at their own hands. The demonstration of plasticity in these systems is important in itself, but this work goes further as it has opened up a new field for experimental investigation.

Gewirtz, prominant on the S-R right wing of child psychology, demonstrates that the reinforcing properties of the word *good* (or *yafeh* for Hebrew children) in a discrimination learning situation depends inversely on prior presentation.

The rather successful use of operant techniques with schizophrenic children is described by Lovaas. In most treatment situations children are rewarded for psychotic behaviour; the child who bangs his head gets most attention. Lovaas punished psychotic behaviour and used a variety of techniques to shape better things. His claims are modest and he quickly gets you on his side by saying that there is more to human behaviour than that contained in reinforcement theory. The most interesting work concerns verbal imitation training. He began by reinforcing all vocalizations and visual fixations of the adult's mouth and then shaped the behaviour to approximate the model more closely. After several weeks of this, many new words were being learnt each day. At this stage he went on to teach a labelling vocabulary and finally to the training of abstract speech. The degree of generalization of the training is surprisingly wide and seems to stand up reasonably well in other situations. Part of this success may be attributed to the use of the parents in the training sessions. His dismissal of Miller's doubts that operant techniques can produce flexible language seem a little naïve. Precise descriptions of the children's verbal behaviour before treatment are not given. Some were echolalics who must have had some familiarity with speaking before training, in any case more than complete mutes.

The increasing influence of Basil Bernstein's ideas in the United States is seen in the study by Hess and Shipman. I.Q. tests and analysis of tasks carried out by mothers with their four-year-old children form the basic data. The families were all Negro and were divided into four social class groups (college educated, skilled manual, manual and manual with father absent). The three lower class groups differed little among themselves but were distinct from the middle class group in terms of the children's performance and its control by the mother. Though this parallels the mothers' I.Q.s, the authors prefer to discuss the results in terms of social class and the lack of choices for ghetto children and their impulsiveness (delay of gratification means no gratification). This implies that "cultural deprivation" is the lot of any black (and white?) child whose parents don't make it to college.

Mavis Hetherington looks at three theories of sex typing. Her results "appear to yield evidence in partial support of the three theories" (and others too, perhaps?). After four solid pages of intercorrelations of parental and child variables, she remarks that the reader may be dazzled by the array of insignificant correlations. This reviewer was.

Suppes, Hyman and Jerman contribute a chapter entirled "Linear structural models for response and latency performance in arithmatic on computor-controlled terminals." The reviewer does not feel competent to comment on this interesting-looking research except to say that it is refreshing to see the learning theorists getting into the classroom.

The editor remarks that volume illustrates that there are many ways of doing good work in child psychology. But then some people may feel that there is more to the game than *doing* psychology. Though this book is not worth buying, if you are interested in where the child psychologists were a few years ago, it might be worth borrowing from the library for a few days.

MARTIN RICHARDS

An Introduction to Animal Behaviour. By Aubrey Manning. London: Arnold. 1967. Pp. viii + 208. 32s. cloth. 21s. paper.

Part of the Contemporary Biology Series, this is an excellent introduction to animal behaviour. Unlike most introductory texts, it will not mislead the beginner into any narrow path, for animal behaviour is considered from a broad approach, involving ethology, psychology and physiology.

The first chapter in this analysis of the organization of behaviour within the individual is *Reflexes and complex behaviour*. In addition to their differences, it stresses their common properties and provides a brief physiological background for what follows. How an animal becomes well-suited to its environment is considered in the *Development of behaviour*. Examples of selective learning in particular species will be of especial interest to psychologists. Although this chapter gives "little encouragement for attempts to classify behaviour into that which is inherited and that which is learnt," the terms are nevertheless used as if a dichotomy existed, with the danger of the beginner accepting the latter. Factors which determine what

an animal does at any given moment include *External stimuli* (sensory capacities, sign stimuli, stimulus filtering) and *Motivation* (from theories of motivation to the physiological basis of motivation). *Conflict behaviour* involves what happens when the smooth course of behaviour is interrupted (threat dispalys, displacement activities, courtship and prolonged conflicts). With *Hormones and behaviour*, a brief physiological basis is again provided before the interactions between hormones and behaviour are considered. The last part of the chapter on insect reproductive behaviour provides a valuable complement to the previous discussion: "it shows that animals with a completely different structure and evolution from the vertebrates nevertheless use similar methods to co-ordinate their behaviour with their reproductive physiology." The following chapter, *Evolution*, concerns genes and behaviour, the micro-evolution of behaviour, and sexual isolation, concluding that "natural selection is opportunistic and operates upon whole animals not upon parts." The last two chapters on *Learning* deal with the classification of different types of learning, the comparative study of learning, and the physical basis of learning. While the classification of learning is difficult to relate to experimental operations and rather outdated, one would like to see sections particularly relevant to the previous chapters expanded (e.g. comparative learning expanded to give a further idea of just what sorts of comparisons are meaningful).

Although presenting such a variety of material on such a variety of animals, the author offers no superficial solutions. He provides clear descriptions of experimental procedures and points out complications in interpretation. Animal behaviour is presented as an intriguing subject requiring further investigation.

JOAN G. STEVENSON

Thinking and Reasoning. Edited by P. C. Wason and P. N. Johnson-Laird. Harmondsworth: Penguin Books Ltd. 1968. Pp. 431. 8s. 6d.

If Dr. Wason and Dr. Johnson-Laird are looking for more material for tests of logical abilities, they ought to try this statement: "All the experiments in our book of selected readings are about problem solving by human subjects. Our book is called 'Thinking and Reasoning'. Therefore human subjects only think and reason in problem solving experiments: they do not reason or even think in experiments on verbal learning, verbal recall, probability learning, perceptual constancy or vigilance." In fact it seems that, in a manner peculiarly appropriate to their own research interests, these editors have defined the subject matter of their book entirely by the exclusion of negative instances. The excluded negative instances are all those experiments in which some form of hypothesis formation is studied, but which nevertheless may be included in a more established category, such as human learning or perception. What is left is human problem solving, and since many students might rely on such a collection as a definition of the content of an area of research, "Human Problem Solving" is what this book should have been called.

If this collection is to be judged on how it manages to present an accurate and coherent picture of research on problem solving, then it must be judged, with one notable exception, to be a success. It has six sections, and three of these are excellent. These three concern Deductive and Inductive Reasoning and Matching Problems. Each section contains an account of a specific area of research in which reports of experimental progress are nicely balanced with discussion of specific problems of experimental design. Of the remaining three sections, two raise certain problems, although they both contain much that is interesting. One can question, for example, in the section on computer simultation why the ARGUS program was included and the EPAM model left out. In another section of which the title is actually "Problem Solving," one can detect signs that the editors are functionally fixed on the topic of functional fixedness. Four out of six papers make the well-worn point that experience can hinder original solutions to problems. One of these might have been abandoned in favour of Yarbus' account of how eye movement studies can be used to detect the effects of previous experience on the way information is assessed in a new situation. It is in the sixth section on development that the most striking imbalance is to be found. This section is devoted entirely to Piaget and his colleagues and to Bruner. Piaget's position cannot be questioned, although the selection of his work here is very odd. Only fleeting

references are allowed to the classic conservation and seriation experiments, and a great deal of space is devoted to his more dubious and less replicable work on class inclusion. Bruner's place is more questionable, since most of his work in this field is derivative and he consistently fails to consider, let alone to eliminate, alternative hypotheses (suitable case, one would have thought, for Dr. Wason). What really is surprising here is the omission in this section and even in the suggestions for further reading of any mention of the major Russian work in this area, the work of Luria, Zaporhzets, Liublinskaya, Zinchenko and Lomov. Many accounts of these exist in English. Anyone interested in this section would do well also to look further afield.

Each section is preceded by a brief introduction, but as in most books of readings the editors give no account of what they actually think about the articles which they have chosen. It is my opinion that there should be a rule that editors should at least say whether they choose papers because they are good, or because they are representative, or both. This might help the reader. It would certainly make reviewers' lives a great deal easier.

P. E. Bryant

Les Troubles de la Résolution de Problémes. By A. R. Luria and L. S. Tsvetkova. Paris: Gauthier-Villars. 1967. Pp. 271.

Neuropsychological Symptoms and Syndromes in Lesions of the Occipital Lobe and the Adjacent Areas. By I. Gloning, K. Gloning and H. Hoff. Paris: Gauthier-Villars. 1968. Pp. 98.

Performances in Aphasia. A Neurodynamical, Diagnostic and Psychological Study. By A. Kreindler and A. Fradis. Paris: Gauthier-Villars. 1968. Pp. 261.

These three volumes, together with a French translation of Hermelin and O'Connor's *Speech and Thought in Severe Subnormality*, are the first to appear in a new series *Collection Neuropsychologia: Monographies de Neuropsychologie* under the Editorship of Professor Henry Hécaen. The Luria-Tsvetkova book is concerned with the analysis of problem-solving defects in cases of frontal lesion and has been excellently translated by Dr. R. Lhermitte. The Glonings and Hoff provide a clinical and statistical analysis of 241 cases of posterior hemisphere lesions, verified anatomically either at operation or autopsy. Kreindeer and Fradis report new chemical and experimental studies in aphasia. This series should be of particular interest to experimental psychologists and others who have occasion to work in the neurological field.

O. L. Zangwill

Hearing Mechanisms in Vertebrates: A CIBA Foundation Symposium. Edited by A. V. S. de Rueck and Julie Knight. London: Churchill. 1968. Pp. xi + 320. 65s.

The publicity for this volume describes it as of interest to neurophysiologists, anatomists, neurologists and otologists. The poor psychologist is not invited. Nor was he, indeed, to the symposium that the book reports. This in itself is an interesting fact. Another recently published book on audition, Whitfield's *The Auditory Pathway*, conspicuously omits reference to psychological evidence. Do the physiologists no longer need psychological observations now they can get inside the system with micro-electrodes and are beginning to sort it out? Scientifically oriented psychologists have long favoured working in an area where the relation between results of behavioural experiments and plausible physiological mechanisms was reasonably close. Have they died out or been given the sack?

In fact they are alive and producing excellent material at a great rate. In a decade the Journal of the Acoustical Society of America has expanded threefold, and the psychoacousticians have more than held on to their share of the total. The understanding of the mechanisms of masking, frequency discrimination, localization, fatigue and loudness perception has increased considerably. The relationship between the psychological results and known or verifiable physiological mechanisms continues to be close. One is tempted to conclude that progress has been too fast for the physiologists to keep up. It might be safer to conclude that this is a predominantly European symposium and that psychoacoustics has not had such a big impact in Europe as in America. These are fairly uncontroversial facts.

But to the book. It contains fourteen presentations in five groups: Structure and Function of Hearing Organs in Non-mammalian Vertebrates; Structure and Function in Mammals; Afferents and Efferents in the Auditory Nerve; Spatial Localization; Central Mechanisms. The two most interesting papers are a neuro-anatomical one by Spoendlin and a neurophysiological one by Fex, on the microanatomy of the organ of corti, and on the role of the efferents to the cochlea, respectively. It appears we are now half way to knowing how the nerve cells are triggered by the hair cells, and what the bundles of efferent fibres do.

One of the most valuable parts of the book is the report of discussions. Typically with such small high-powered groups, these discussions contain a wealth of offbeat information that one vaguely knows about but cannot lay ones hands on—information that is problem-oriented rather than presentation-oriented.

The volume has an appealing aroma of high-quality paper and is full of photographs and diagrams. Despite its lack of psychoacoustics it will be a standard reference in hearing for many years to come. And rightly so.

MARK P. HAGGARD

Language: Selected Readings. Edited by R. C. Oldfield and J. C. Marshall. Harmondsworth: Penguin Books. 1968. Pp. 392. 8s. 6d.

As books of readings go, this is pretty good, certainly streets ahead of a similar selection previously evaluated by this reviewer. Each paper has a claim to fame and inclusion, although a different pair of editors would probably have omitted some and included others. The introductions are short and to the point. One would have preferred a text, but this book is definitely of use in advanced teaching.

MARK P. HAGGARD

Studies in Psychiatric Art: Its Psycho-Dynamics, Therapeutic Value and Relationship to Modern Art. By R. W. Pickford. Springfield, Illinois: Thomas. 1967. Pp. xix + 348. $14.00.

Professor Pickford has collected together a number of his articles and papers in the general field of art and psychopathology and added a certain amount of unpublished material. As might be expected from this author, the point of view is Freudian, though not narrowly so—indeed one chapter betrays considerable sympathy with Jung. The result, though somewhat lacking in unity and theme, is an interesting and in patches extremely perceptive study.

O. L. ZANGWILL

Aggression and Defense. Edited by C. D. Clemente and D. B. Lindsley. Los Angeles: University of California Press. London: Cambridge University Press. 1968. Pp. xv + 361. £7 2s. 6d.

This book is the proceedings of a conference held in California in November, 1965, with participants from a wide range of disciplines. The initial similarity between this and an earlier book, Carthy and Ebling (1964), proves deceptive, and if the reader is not deterred by the frontispiece he may find many of the chapters useful for the data they contain or as reviews of areas of research. Other chapters will be much less useful. The increasing frequency with which books such as this are produced raises the question of whether they have a merit other than that of their constituent papers. I think that they may have where the discussions show which are superficial and which are basic points of disagreement between authors with apparently opposing views, e.g. I. Eibl-Eibesfeldt and J. P. Scott in this volume.

In the opening paper, L. S. B. Leakey suggests that weapons were first used by pre-man when he turned to a scavenging existence, to drive other scavengers from the kill: later they were used to kill prey, and by a natural progression, to kill his fellows. This is followed by papers on animal behaviour and the physiology of agressive behaviour. The Cetacea are discussed by K. S. Norris. The widespread occurrence of ritualized fighting

in animals which possess dangerous weapons suggests that strong selection pressures operate to retain the use of aggressive behaviour as a spacing out mechanism, and I. Eibl-Eibesfeldt and S. A. Barnett discuss the way in which species resolve conflicts without the use of harmful weapons. Neither author attempts a comprehensive review of the literature on animal fighting, and there is little mention of fish behaviour, where much of the quantitative work has been performed, or of the relationship between courtship and fighting displays. Eibl-Eibesfeldt also deals with the effect of isolation on aggressive behaviour, and this section shows clearly how his (Lorenzian) view of aggression differs from that of e.g. J. P. Scott. Reaction to a strange animal with the species-specific motor patterns of attack does not require the previous experience of conspecifics: hence fighting is not solely learned behaviour. Studies showing that the opportunity to attack is reinforcing are interpreted as evidence of an "urge to fight." Finally, examples of fluctuations in the threshold for attack are discussed. If a male Cichlid *Etroplus maculatus* could attack other males through a glass plate, few attacks were made on the mate, while if the pair were visually isolated, the mate was attacked and killed. Eibl-Eibesfeldt suggests that this is the result of damming up the male's aggression. This increase in responsiveness is the vital point in his argument for spontaneity, and it is not clear whether it is due to the removal of a short term inhibition, or whether it is a long term, continuous threshold lowering. Heiligenberg's work on *Pelmatochromis*, also quoted, shows that the lowering of the threshold for aggressive behaviour is not an invariable consequence of isolation in Cichlids, and it seems important that these increases in responsiveness with time should be investigated by those who hold that aggression is not spontaneous, as well as by those who hold that it is.

S. Kawamura shows how complex aggressive behaviour may be in troops of Japanese macaques. Thus, a monkey may attack an inoffensive bystander to divert an attack from itself (by inducing its own attacker to join in this attack), or to apply psychological pressure to a leader (by demonstrating that it lacks the superiority to punish this attack as its status demands it should). By joining in and leading the attacks of a dominant animal, a monkey may obtain the other's friendship, and hence raise its own status. Moreover, there are differences between troops not only in tolerance of proximity, but in the possession of particular ritualized behaviour patterns.

This chapter shows clearly how inadequate an analysis of attacks in simple stimulus-response terms may be. A chapter by J. M. R. Delgado shows how by radiostimulating Rhesus monkeys with chronically implanted electrodes in a social group, it may be possible to study such complex behaviour physiologically. Brain loci at which stimuli evoked stereotyped gestures and grimaces which might appear aggressive, but which were not socially directed, could be separated from those producing, as an after-effect of stimulation, well co-ordinated attacks of variable form directed animals with which relations were normally unfriendly. The contribution of more conventional ablation and stimulation studies to the problem of the neural organization of aggressive behaviour is reviewed at length by B. Kaada. A. B. Rothballer eschews psychopharmacology and adopts an ecological approach when discussing the relations between neurohumours (which he defines broadly) and aggressive behaviour. Interspecific differences must be borne in mind in a search for the physiological correlates of aggressive behaviour. Rothballer cites evidence to show that while in species in which territory formation and breeding occur together, testosterone controls the aggressive behaviour, LH may be important in species in which territories are claimed before much testis growth has occurred: he does not point out that the experiments in this area urgently need repeating and extending. Changes in hormone levels consequent on aggressive behaviour are then discussed, and B. L. Welch adds a comment on the relations between catecholamines and fighting.

The four final papers, on human behaviour, differ widely. L. Berkowitz reviews a small body of data cues provided by a target associated with aggression in the individual's past which increases the aggression elicited. Cognitive processes may be modified in an attempt to provide a justification for these responses. H. W. Brosin provides a historical review of ways in which aggression is controlled, from a psychiatrist's point of view, and

the book ends with chapters by S. C. Plog on the methods used to study the Watts riots of 1965, and by H. D. Lasswell on the social and political framework of war and peace. There are subject and name indices.

For the reader who wants a handy source of references to a restricted field, the wide scope of this book will be a disadvantage, but the wide range of views expressed is welcome in the light of the one-sided theses propounded in some recent, and very popular, books on aggression.

P. G. Caryl

Human Aggression. By Anthony Storr. London: Allen Lane The Penguin Press. 1968. Pp. vi + 127. 25s.

This book is evidently intended for the naïve reader—in the sense in which experimentalists refer to "naïve rats"—that is, it is essentially for those who have not already been exposed to theories of aggression, for it does not have a great deal to say that is new. It is an account of human aggression, as understood by the psycho-analysts. First it is compared with aggressive behaviour in the lower animals; it is then discussed with respect to infants, children and adults, and then with respect to sexual relationships. In the later chapters, aggression is discussed in the depressive, the schizoid, the paranoid and the psychopath. The last chapter is on "Ways of reducing hostility."

The main features are the repeated stress (i) on the long period of development required by the human being before he attains physical and emotional maturity: the dependence and vulnerability engendered by this is thought to be one of the reasons underlying the wanton aggression and cruelty which is found in *homo sapiens* alone. (ii) Dr. Storr is at pains to point out that aggression has good features as well as deplorable ones: he evidently considers it as potent a force as the sexual impulse in artistic and intellectual creativity. This leads to (iii) the impression given by Dr. Storr that women constitute an inferior minority of the population whose only claim to creativity is in the strictly biological field. That this is no overstatement may be seen in his consolatory message to womankind: "But it is equally vital that we should be cherished and fed, and that we should reproduce ourselves. Women have no need to compete with men; for what they alone can do is the more essential."

This does not seem wholly compatible with the (readily acceptable) statement in the final chapter that the reduction of world population "is the most important single step which can be taken by mankind to reduce hostile tension." This chapter contains some other practical suggestions for decreasing international tension, but it neglects to mention the substantial financial gain made by a few as a result of war.

A. W. Heim

A Textbook of Physiological Psychology. By Sebastian P. Grossman. London and New York: Wiley. 1967. Pp. xix + 932. 113s.

Dr. Grossman has written an excellent comprehensive text for physiological psychology which deserves a place on the reference shelves of all psychology libraries. It provides a wide and accurate introduction and review of almost all the major aspects of physiological psychology. It should be useful to lecturers and research workers and should certainly be strongly recommended to students with physiological interests.

The text is divided into four parts, Part I (174 pages) consists of the biophysics, basic electrophysiology and anatomy of the nervous system. Part II (132 pages) is concerned with sensory processing, sensory-motor integration and the reticular activating system. Part III (285 pages) describes a wide range of experimental work on motivational mechanisms. Part IV (258 pages) is concerned with the plasticity of the organism's response repertoire and mechanisms of learning.

The depth of coverage of these subjects varies greatly. For example, Part III on motivational mechanisms includes extremely comprehensive chapters on Hunger, Thirst, Sexual Behaviour, Emotional Behaviour and Self-stimulation. This after all is Dr.

Grossman's own field of research and these chapters will undoubtedly be of great value to students and research workers alike. It seems a pity that the author's personal views of motivational mechanisms were not more apparent as he has been in the forefront of this field for a number of years.

On the other hand, Chapter 5 on the reticular formation is very limited in its coverage. The mechanisms of sleep and paradoxical sleep are barely mentioned, which is a sad omission in view of the enormous current interest in these subjects.

It would obviously be impossible in one text to give detailed coverage to all the subjects listed in the index and possibly it would have been wise to use greater selectivity. For example the first 177 pages describe the basic biophysics, electrophysiology and anatomy of the nervous system in more detail than is perhaps necessary, especially in view of the fact that excellent physiology and anatomy texts exist to provide the student with this information. Also, it seems difficult to justify the inclusion of part of Chapter 3 on vision, which takes the reader through the most elementary psychophysics and description of visual illusions before discussing the electrophysiology of the visual system. On the other hand, the auditory and somaesthetic systems are not described in any detail and perhaps a more general appraisal of the sensory processing mechanisms would have produced a more global and relevant approach to the senses for the physiological psychologist.

From a different point of view, it is unfortunate that the final chapter on biochemical methods and learning is not more critical. It is admirable that this subject is introduced with some basic biochemical facts about nucleic acids and their synthesis but unfortunate that the controversial experiments themselves, which attempt to relate changes in nucleic acid synthesis with learning and memory were presented without, what many consider, the very necessary caution.

It is debatable what function a textbook fulfils, but in a subject like physiological psychology it should be more than a statement of the facts from the contributory disciplines. This textbook is presumably aimed at final year undergraduates and postgraduates and considering its audience a little more interpretation and synthesis by the author would have made the enormous amount of experimental data presented both more palatable and exciting.

Nevertheless, Dr. Grossman has presented a massive amount of material accurately, the bibliography is extensive and will be very useful to student, lecturer and researcher. The text is well produced and the information is arranged clearly under numerous headings and sub-headings. The photographic work is of a high standard with informative captions. The usefulness of the illustrations is marred only by the failure in the written text to refer by number or letter to the diagrams. This is particularly noticeable in Part III of the book, which is in many other ways excellent.

S. D. Iversen

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PROCEEDINGS OF THE EXPERIMENTAL PSYCHOLOGY SOCIETY, 1968

28th–29th March 1968. Meeting at Cambridge.

1st Session: "Some experiments on simultaneous translation," by Mr. D. Gerver.† "Delayed auditory feedback: a limited capacity model," by Mr. D. Salter.† "Temporal uncertainty in choice-reaction experiments," by Dr. D. R. J. Laming.

2nd Session: "Disorders of visual perception in patients with localized cerebral lesions," by Dr. Elizabeth Warrington. "An automated technique for the study of learning ability in psychogeriatric patients," by Dr. A. W. H. Buffery, Mr. C. J. Cheney† and Dr. J. L. Gedye. "Speech and memory during anaesthetization of one or other cerebral hemisphere," by Dr. Brenda Milner.

3rd Session: "Maintenance of responding by a fixed-interval schedule of electric-shock presentation in squirrel monkeys (*Saimiri sciurens*)," by Dr. R. G. A. Stretch. "A new theory of visual spatial resolution based on neuro-physiological findings," by Dr. F. W. Campbell, Dr. G. F. Cooper† and Dr. J. G. Robson.†

4th Session: "On the measurement of loudness," by Professor M. Treisman. "The invariance of critical bandwidth," by Dr. M. P. Haggard. "Massed and distributed practice—is it a dead issue?" by Dr. A. D. Baddeley and Miss D. J. A. Longman.† "Anxiety stress and decrements in skill," by Dr. M. Hammerton. "Interaction of verbal instructions and task difficulty in the learning of perceptual-motor skill," by Mr. B. R. Gaines.†

16th–17th July 1968. Meeting at Nottingham.

1st Session: "Problems of measuring performance in blind mobility," by Dr. J. A. Leonard. "The heart-rate of blind and sighted pedestrians on an unfamiliar route," by Mr. R. Wycherley† and Miss B. Nicklin.† "Physiological measures of stress," by Dr. M. Hammerton. "Baseline determinants of conditioned suppression, comparing two research strategies," by Dr. D. E. Blackman.

2nd Session: "Topological representations and the structure of verbal problems," by Mr. D. Woods.† "Dissociated deficits in right hemisphere lesions," by Dr. Freda Newcombe.† "The synaptic basis of memory," by Dr. J. A. Deutsch.

3rd Session: "Pre-perceptual acoustic storage," by Dr. J. Morton and Dr. R. G. Crowder.† "Recognition and number of choices: A-index and d′ comparisons," by Professor J. Brown. "A decision theory approach to signal detection and its application to detection latencies: some further developments," by Dr. G. Wolfendale.†

4th Session: "Interaction of visual stimuli: Benham's top and Bidwell's disc," by Dr. A. H. Gregory. "Experiments on timing in natural speech," by Dr. A. W. F. Huggins.† "Speed-error trade-off in serial choice reaction tasks," by Mr. D. Hale.† "Temporal uncertainty in choice-reaction experiments—2", by Dr. D. R. J. Laming. "Camouflage and visual search," by Professor C. I. Howarth and Mr. J. R. Bloomfield.† "Stimulus uncertainty in visual detection and recognition," by Dr. M. Matthews† and Mr. J. Kemp.†

† By invitation

6th–7th January 1969. Annual General Meeting at Bedford College, London.

1st Session: "Serial and parallel processing in pattern recognition," by Mr. A. J. Marcel.† "Some problems in the hemispheric control of reaction time," by Dr. S. J. Dimond.† "Attentional narrowing in loud noise," by Mr. G. R. J. Hockey.† "A neurophysiological model of beta apparent movement and some relevant psychological data," by Mr. J. P. Frisby.†

2nd Session: "Intra-trial interference in short-term memory," by Dr. H. Loess. "Specific impairment of auditory verbal short-term memory," by Dr. Elizabeth Warrington and Dr. T. Shallice. "The assessment and analysis of handedness," by Professor R. C. Oldfield.

3rd Session: "Adaptation of divers to size-distance distortion under water," by Dr. Helen E. Ross, Mr. S. S. Fanklin† and Mr. G. Weltman.† "Some experiments on pitch perception and their relation to frequency control in the human voice," by Mr. A. Rostron.† "Effects of frontal cortex lesions on the development of visual discrimination learning set in the monkey," by Dr. J. M. Oxbury. "Inter-ocular transfer in the hooded rat," by Dr. L. Nadel.†

4th Session: "The role of the cerebral cortex in passive avoidance learning," by Mr. D. Kleinman.† "The hemidecorticate learning deficit," by Dr. H. C. Plotkin.† "A parametric study of lateralization in the functional split-brain," by Dr. I. S. Russell. "On the relationship between letter names and superordinate categories," by Dr. M. I. Posner. "The effect of task difficulty on the relative speeds of correct and incorrect responses in an identification task," by Mr. J. Wilding.†

† By invitation

Committee, 1969: *President:* Professor P. H. Venables.

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PSYCHOLINGUISTICS

EXPERIMENTS IN SPONTANEOUS SPEECH

FRIEDA GOLDMAN EISLER

Psycholinguistics Research Unit, Department of Phonetics, University College London, England

1968, viii + 170 pp., 50s.

This volume presents and discusses experiments aimed at increasing an understanding of the generative processes involved in the production of speech. Special emphasis is placed upon the measurement of extralinguistic phenomena in spontaneous speech but structural aspects have also been studied. The main parameters were hesitation pauses, breathing in speech and sentence complexity. The orientation is towards their interpretation as indicators of cognitive processes and of the higher nervous activity that accompanies the act of speech production.

These findings are relevant to all those workers who are interested in evidence about changes in the states of speakers in relation to the properties of the speech and language generated.

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The Problem. Introduction. Some Facts Concerning Pauses in Speech. The Significance of Hesitation Pauses. The Semantic Determination of Pauses and Spontaneity. Complexity of Cognitive Operations, Information and Hesitation. The Cognitive Status of Syntactic Operations. Sequential Temporal Patterns in Speech. Temporal Patterns of Cognitive Activity and Breath Control in Speech. The Significance of Breathing in Speech. Experimental Validation of Speech-breathing Parameters. Conclusion. Appendices. Notes. References. Author Index. Subject Index.

Academic Press **London and New York**

Berkeley Square House, Berkeley Square, London, W1X 6BA, England.

111 Fifth Avenue, New York, N.Y. 10003, U.S.A.

Information Theory of Choice-Reaction Times

D. R. J. LAMING
Psychological Laboratory, University of Cambridge, England

1968,
x + 172 pp.,
63s.

This book represents an important advance towards a comprehensive quantitative theory of choice-reaction times. The central thesis is simple: the response on each trial of a choice-reaction experiment expresses all the information about the signal that the subject has collected. The choice-reaction process should therefore be similar to Stone's model based on the sequential probability ratio test. Certain non-parametric predictions from the theory are confirmed by experiment.
There are review chapters covering Communication Theory and other mathematical models for choice-reaction time, definitive reports of seven new experiments, a detailed mathematical development and appraisal of Information Theory applied to choice-reaction times, and some important and original empirical results on sequential constraints in choice-reaction experiments. An answer is also given to the question: What assumptions about the state of nature are implicit in this application of Information Theory to human behaviour?"

This book is intended primarily for those engaged in research in choice-reaction times and related topics. But this development of Information Theory and the empirical results have a wider importance than this. They will be found relevant by anyone interested in the study of human behaviour in terms of the processing of information.

Chapter Headings

The Explanation of Choice-Reaction Times in Terms of Communication Theory. Alternative Models for Choice-Reaction Times. The Random Walk Model. The Apparatus and the Experimental Procedure. The Experiments. An appraisal of the Random Walk Model. A Critical Examination of Certain Alternative Models for Choice-Reaction Time. Sequential Analysis. Future Developments. Appendix A: The Two-Choice Theory. Appendix B: The Multi-Choice Theory. Appendix C: Two Methods of Analysis. References. Author Index. Subject Index.

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ANIMAL DISCRIMINATION LEARNING

Edited by R. M. Gilbert and N. S. Sutherland

March 1969,
xvi + 501
pp., 135s.

Alcoholism and Drug Addiction Research Foundation, Toronto, Canada
Laboratory of Experimental Psychology, University of Sussex, England

Although this book grew out of a conference held at the University of Sussex, it is only loosely based on the conference proceedings. Most of the chapters were extensively revised after discussion both at the conference and with the Editors; several further chapters were specially commissioned from authors who were not present at the conference. Thus, the Editors are able to present a coherent account of most of the main problems in animal discrimination learning.

The work contains both original material and reviews of many recent developments. Considerable emphasis is placed on the role of selective attention. Several chapters are devoted to the important but neglected problem of interspecies differences in discrimination learning. Other topics covered in some detail include generalization, behavioural contrast and peak shift, the discrimination of time intervals by animals, response strategies, and aversive schedules. One chapter is devoted to visual pattern recognition, and another to issues in the physiology of discrimination learning.

The book will be of importance to experimental and physiological psychologists, to comparative ethnologists, to psychopharmacologists and to all who study animal behaviour.

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